

ALIENATION, AMBIVALENCE, AND SCOREKEEPERS: THREE ESSAYS ON PUBLIC  
OPINION FORMATION IN AMERICAN POLITICS

Scott Christopher O'Brien

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Approved by:

Thomas M. Carsey

Pamela J. Conover

Michael B. MacKuen

Kevin T. McGuire

James A. Stimson

## ABSTRACT

SCOTT C. O'BRIEN: Alienation, Ambivalence, and Scorekeepers: Three Essays on Public Opinion Formation in American Politics  
(Under the direction of Thomas M. Carsey)

This dissertation is composed of three chapters, each of which explores public opinion formation in American politics:

The first chapter, titled “Polarization, Alienation, and Trust in Government,” introduces *perceived polarization* (the ideological distance a citizen perceives between the parties) and *ideological alienation* (the perceived ideological distance between a citizen and the closest party) as process-oriented predictors of trust in government. Using the American National Election Studies (ANES) Cumulative Data File, I find that polarization and alienation both explain trust. I find that these relationships are robust across both time and trust measures.

The second chapter, “Left Behind: Competitive Nominations and Comparative Candidate Ambivalence,” investigates *comparative candidate ambivalence* (simultaneously holding positive attitudes toward competing general election candidates) as a possible competitive nomination carryover effect on thwarted voters. I draw on motivated reasoning theory to argue that unlike other groups, thwarted voters should gather positive information about *both* party nominees during the general election due to (1) “sour grapes” toward their party; and (2) party loyalty-driven pressure to support their party nominee. I test my theory in the context of the competitive 2008 Democratic nomination contest between Hillary Clinton and Barack Obama. Using the 2008-09 ANES Panel Study, I investigate differences in cross-sectional and

longitudinal ambivalence between Clinton voters and five other electoral groups. My results are promising, but limited due to low group sample sizes.

The third chapter, “Who Are the Scorekeepers? Sophisticated Independents and Economic Perceptions,” attempts to validate Stimson’s (2004) theory that there is a small group of politically well-informed but non-partisan citizens who evaluate the economy objectively. I operationalize these “scorekeepers” as politically sophisticated independents and divide the ANES sample into four groups: in-partisans, out-partisans, unsophisticated independents, and sophisticated independents. On the micro level, I find evidence that partisans’ economic retrospections are biased relative to independents’ retrospections. On the macro level, I find that independents mirror the retrospections sample mean particularly well, but partisans are biased. Lastly, I compare each group’s predicted probabilities of several retrospection responses across Gross Domestic Product growth levels and find that sophisticated independents’ retrospections are most responsive to objective economic conditions.

To Ron Rapoport, for making me believe I could do it.

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## I. POLARIZATION, ALIENATION, AND TRUST IN GOVERNMENT

Political trust reflects the perceived competence, fairness, and responsiveness of government. It measures the public's confidence in government to do what is right as well as perceptions of how efficient, corrupt, and wasteful it is. Scholars regard trust as a vital sign of a democracy's health (Dahl 1971; Hetherington 2005). They generally agree that trust is necessary to legitimize government policies, promote citizen compliance with the law, and maintain regime stability (Gamson 1968; Barber 1983; Tyler 1990). Without sufficient trust, government must resort to coercive practices to ensure its decisions are accepted and implemented (Levi 1997). Political observers worry that low trust diminishes political participation. While limited distrust toward government may be healthy in a deliberative democracy (Barber 1983; Warren 1999), if trust vanishes altogether, so might the regime (Easton 1965; Gamson 1968; Hetherington 1998; Hardin 1999).

Political observers expressed anxiety about the consequences of a well-documented erosion of trust following the civil rights movement, Watergate, and Vietnam War and again after trust unexpectedly plummeted in the early 1990s (e.g., Abramson 1983; Lipset & Schneider 1987; Miller & Borelli 1991; Craig 1996; Orren 1997; Citrin & Luks 2001; Hibbing & Theiss-Morse 2001). Declining trust, they argue, potentially leads to concomitant reductions in voter turnout, grassroots lobbying, and other means of political participation (Hibbing & Theiss-Morse 2002). Therefore, it is important to understand the causes of trust.

In this paper, I propose and test two potential influences on trust attitudes: The first is *perceived polarization* -- the ideological gap citizens perceive between the major political parties. Observers have noted increasing separation of what V.O. Key (1964) termed “the parties in government” and “the parties as organizations” along the traditional left-right ideological spectrum. Since 1980, the Democratic Party has become more liberal and the Republican Party has become more conservative, all else equal (Layman, Carsey, & Horowitz 2006). To date, only limited scholarly work has investigated a possible relationship between polarization and trust. Though some argue that polarization has driven voters (especially moderates) away from political engagement, this conclusion is more anecdotally than empirically-supported (see King 1997; Hibbing & Theiss-Morse 2001; Fiorina, Abrams, & Pope 2006; Hetherington 2008). This paper more rigorously investigates whether the ideological gap citizens perceive between the parties has meaningful impacts on trust attitudes.<sup>1</sup>

The second potential determinant of trust I investigate is *ideological alienation* from the major political parties.<sup>2</sup> I define ideological alienation as the Euclidian distance between citizens’ ideological self-placement and their placement of the nearest political party. Political scientists have primarily investigated ideological alienation from candidates as an explanation for low election turnout levels; the more ideological distance between a citizen and the closest political party, the more likely he or she is to abstain from voting (Zipp 1985; Adams & Merrill 2003; Plane & Gershtenson 2004). In this paper, I make the case that ideological alienation from the parties may explain trust attitudes as well.

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<sup>1</sup> I analyze *perceived* polarization because citizens’ perceptions of polarization should drive their trust attitudes regardless of what is actually happening to the parties. Polarization exists in a citizen’s mind only to the extent that he or she perceives ideological distance between the parties. In other words, perceptions should drive trust attitudes.

<sup>2</sup> This should not be confused with the more vaguely defined concept of “political alienation” explored by the sociology literature in the 1970s. I am solely concerned with spatial distance between citizens and the parties.

In what follows, I use the American National Election Studies (ANES) cumulative file to test a model incorporating both perceived polarization and ideological alienation as predictors of trust. I proceed as follows: First, I review the existing literature on trust, polarization, and alienation as well as their limited intersections. Next, I formally define the key concepts in my analysis, lay out the theoretical reasoning behind my expected findings, and develop testable hypotheses. Then, I discuss my model, data, and estimation methods. Finally, I report my results and reach several substantive conclusions about them.

### **The Framework of Trust**

David Easton's (1965) seminal work on political institutions defines trust as a form of support -- the evaluative inclination citizens hold toward political systems. He delineates two aspects of political systems that determine the level of trust the citizenry has in government: *Specific* support captures "attitudes toward an institution based on the fulfillment of demands for particular policies or actions" (Easton 1965, 273). Such support is derived from the actions of policymakers. Thus, it is the type of support captured by performance variables such as presidential approval. Easton argues that distrust toward the authorities can be remedied easily through electoral replacement. As a result, it has been of less concern to scholars than the other component, *diffuse* support.

Diffuse support refers to a "reservoir of favorable attitudes or goodwill that helps members to accept or tolerate outputs to which they are opposed or the effects of which they see as damaging to their wants" (Easton 1965, 273). This type of support comes from the reverence an institution elicits and the responsiveness of the political process, independent of those in power. As a result, it tends to remain more stable than specific support over time. Scholars are more concerned with diffuse distrust than specific distrust because it has more

serious implications for democracy – since it cannot be remedied through electoral replacement, it could potentially fester over time and eventually threaten a regime’s existence.

As a result of Easton’s work, the literature on trust in government has evolved around the diffuse (process-oriented) and specific (performance-oriented) support framework. Scholars used it to debate the ramifications of a major decline in trust during the late 1960s and early 1970s. On one side, Miller (1974a; 1974b) argued that the decline was institutional and hence the effects were serious and likely to endure without significant institutional changes. Conversely, Citrin (1974) claimed the decline stemmed from performance evaluations and not the institutions of government more broadly. Therefore, the effects were less serious because they were correctable via electoral replacement of incumbents.

Trust research has focused on performance-oriented causes of trust (Nye 1997). Presidential and, in particular, congressional approval powerfully predict trust (Feldman 1983; Williams 1985; Citrin & Green 1986; Erber & Lau 1990; Craig 1993; Hibbing & Theiss-Morse 1995; Hetherington 1998, Chanley, Rudolph, & Rahn 2000; Citrin & Luks 2001). Scandals and perceived government corruption decrease trust (Garment 1991; Orren 1997; Pew 1998). Another powerful factor is national economic performance, economic perceptions (Citrin & Green 1986; Weatherford 1987; Miller & Borrelli 1991; Lawrence 1997; Hetherington 1998; Chanley, Rudolph, & Rahn 2000; Citrin & Luks 2001; Hetherington & Rudolph 2008), and to a lesser degree, personal economic problems such as unemployment (Nye & Zelikow 1997). Finally, research indicates that declining trust can be attributed in part to moral discontent with social ills such as crime and poverty (Craig 1993; Mansbridge 1997; Hetherington 1998; Pew 1998).

Despite these established performance-oriented causes, the fact that the decline in trust has persisted across decades of government turnover indicates that there are likely unidentified

process-oriented influences at work. In one of the few research efforts to tackle these influences, Keele (2007) found a positive over-time macro-level relationship between social capital and trust. To date, no work has attempted to isolate individual-level process-oriented explanations of trust. I propose and test two such potential explanations in this paper.

### *Party Polarization*

The decline in trust in the early 1990s lacked any obvious explanation such as a poor economy or an unpopular war or scandal, so scholars directed new energy toward better understanding trust. At about the same time, they became normatively concerned about party polarization and its effects on democracy. In simple terms, party polarization is the ideological gap between the parties. The scholarly consensus is that beginning in the 1980s, the parties in government and parties as organizations began polarizing. The trend was exemplified by the Newt Gingrich-led Republican takeover of Congress in 1994 that resulted in a government shutdown after severe partisan bickering.

The bulk of the work on institutional polarization has focused on Congress (see Layman & Carsey 2002). In particular, studies have implicated the partisan realignment of the South (Rohde 1991; Hood, Kidd, & Morris 1999; Jacobson 2000; Weisberg 2002), increasing ideological dissimilarity between, and homogeneity within, parties in Congress (Abramowitz & Saunders 1998; Hood, Kidd, & Morris 1999; Jacobson 2000; 2005; Weisberg 2002; Fleisher & Bond 2004), changing demographics of congressional districts and the American public, and increasing party activist influence as causes of congressional party polarization (McCarty, Poole, & Rosenthal 1997; Gimpel 1999; Stonecash, Brewer, & Mariani 2003; Oppenheimer 2005; Layman et al. 2010). A second line of research has found evidence of increased partisanship in

party leadership, strategy, and rules within Congress (Rohde 1991; Cox & McCubbins 1993; Sinclair 1995; 2000; Snyder & Groseclose 2000; Theriault 2006).

The evidence for polarization in the electorate is mixed; however, the balance of research suggests that the public has also become more deeply divided in recent decades, but not to the same degree as elites have (DiMaggio, Evans, & Bryson 1996; Levine, Carmines, & Huckfeldt 1997; Abramowitz & Saunders 1998; Jacobson 2000; 2005; Fleisher & Bond 2001; Layman & Carsey 2002; Weisberg 2002; Stonecash, Brewer, & Mariani 2003). However, Fiorina, Abrams, & Pope (2006) caution that the degree to which the parties have polarized in the electorate is significantly overstated in the academic and popular literature. They argue that while the political class has polarized in recent years, most citizens have remained relatively moderate. Layman, Carsey, & Horowitz (2006) and Abramowitz & Saunders (2008) respond that Fiorina, Abrams, & Pope's (2006) findings, while important, still reveal statistically significant separation of the parties in the electorate since 1980, particularly with regard to issue positions on abortion and homosexuality.

### *Ideological Alienation*

Political alienation can be broadly defined as the degree of estrangement one feels from the political process. It came into vogue as a sociological research topic during the early 1970s in the aftermath of massive social and political unrest over race relations, Watergate, and the Vietnam War. Sociologists feared that these socially traumatic events would isolate the public from the political process. Their work often treated government distrust and political efficacy as the primary indicators of alienation (see Aberbach 1969; Miller 1974a, Miller 1974b; Citrin 1974; Macke 1979; Rahn, Kroeger, & Kite 1996). Meanwhile, a separate and substantial body of work in political science's spatial modeling literature conceptualized alienation as the ideological



distance between a citizen and the closest candidate (e.g., Davis, Hinich, & Ordeshook 1970; Brody & Page 1973; Enelow & Hinich 1984). Ideological alienation is distinct from sociologists' conception of political alienation because, while both concepts purport to capture a feeling of isolation from the political system, ideological alienation is more narrowly focused on ideological distance from candidates (or, as in this case, parties) and more metrically precise (measured using Euclidean distance).<sup>3</sup>

Research indicates that citizens are less likely to vote if they are alienated by ideologically distant candidates (e.g., Hinich & Ordeshook 1969; Hinich, Ledyard, & Ordeshook 1972; Zipp 1985; Adams & Merrill 2003; Plane & Gershtenson 2004). In this context, alienation measures the linear distance from a citizen's ideological self-placement to their placement of the nearest candidate (Plane & Gershtenson 2004). The smaller this distance, the less alienated the citizen is and the more likely he or she is to vote. The larger the distance, the more likely he or she is to abstain. The spatial modeling literature typically contrasts alienation with indifference, which is when both candidates are equidistant from the citizen. However, the predicted response is also abstention (Brody & Page 1973). Since ideological distance is a summary measure of many issue-based distances, some might question whether it accurately captures voters' electoral calculus. Hinich & Munger (1994) convincingly argue that the sheer number of electoral issues makes using individual issue assessments prohibitively demanding of cognitive resources for most voters. Therefore, a summary ideological distance measure accurately captures most voters' assessment of their issue-based ideological proximity to candidates.

Though the spatial modeling literature is replete with theoretical depictions of alienation and indifference, there is little empirical work that tests whether they affect actual voting behavior. One such example is Zipp (1985), who finds that alienation from and indifference

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<sup>3</sup> Alienation is such a murky concept in the sociology literature that it has spawned articles dedicated solely to determining its meaning and how it should be measured (e.g., Aberbach 1969).

toward perceived candidate issue positions affects the likelihood of voting in presidential elections from 1968-1980, with indifference exerting a significantly larger impact than alienation. Adams, Dow, & Merrill (2006) reinforce Zipp's findings, modeling alienation and indifference as functions of non-policy variables and empirically testing a formal theory-driven turnout model. Plane & Gershtenson (2004) find similar results for both the probability of individual-level voting and for aggregate-level turnout in midterm Senate elections.

To date, only one analysis has attempted to connect ideological alienation to citizens' attitudes instead of voting behavior: King (1997) conceives of alienation as an individual-level impact of aggregate-level party polarization. He finds that those who distrust government tend to be the most ideologically distant from strong party identifiers. Building on this analysis, I measure ideological alienation from the parties (rather than candidates or strong party identifiers) to see if it improves our understanding of trust.

### **How Polarization and Alienation Might Explain Trust**

There are several ways we might expect polarization to affect trust: First, it might impede government performance. Ostensibly, parties that find themselves on opposite ends of the ideological spectrum will have trouble moving legislation both in Congress and through the president. Parties in government will be less likely to compromise on policy outcomes. The result will be at worst complete gridlock or at best diminished policy output, resulting in perceptions of ineffective government (Hetherington 1998). We might therefore expect citizens to react to poor productivity by trusting government less to "do the right thing" since it is not doing much at all.

Second, polarization may make the tone of political discourse bitter and divisive, causing citizens to lose faith in government. Citizens should find it hard to trust a government whose

leaders cannot engage in civil policy debates. As extreme ideologues shout at one another across an ever-widening ideological divide, citizens may start to become cynical about the government's ability to construct reasoned policy. For example, in a Pew Research Center report, respondents listed the overly partisan nature of government as a prevailing reason they disliked it (Pew 1998). Moreover, experimental research has demonstrated that televised incivility leads to short-term declines in trust (Mutz & Reeves 2005; Forgette & Morris 2006).

Alternatively, it is conceivable that polarization has the opposite effect on trust: Miller (1974a) worried in the 1970s that undifferentiated parties may leave citizens dissatisfied by the lack of clear alternatives, leading to distrust (Miller 1974a). It follows that clearly contrasting choices might make citizens more likely to engage in the political process, leading to higher trust (Hetherington 2001). It is also possible that citizens are comfortable with a certain degree of gridlock because it ensures that the consequences of policy proposals are carefully considered and that only centrist policies survive. Finally, polarization may build trust by ensuring that both sides are energetically represented in policy debates, bitter as they may be.

Thus, I have theoretical support for both a negative and positive relationship between polarization and trust. As a result, I formulate two competing polarization hypotheses: First, my *negative polarization hypothesis* states that, all else equal, as perceived polarization increases, trust should decrease. Second, my *positive polarization hypotheses* states that, all else equal, as perceived polarization increases, trust should increase. This response should occur independently of the citizen's ideological position; it is the perceived gap between the parties that matters, not the citizen's ideological position relative to that gap.<sup>4</sup>

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<sup>4</sup> While alienation and polarization are conceptually distinct, scholars have argued that polarization may lead to greater alienation over time: Since the citizenry has more or less normally distributed ideological preferences (Fiorina, Abrams, & Pope 2006), all else equal, polarizing parties should increase the ideological distance between citizens and the parties (King 1997). This increased distance should, in turn, make the median citizen feel more estranged, or alienated, from the parties that purport to represent their preferences in government (Craig 1996; King

Turning to how ideological alienation should influence trust, alienated citizens should feel excluded from the political discourse, leading to distrustful attitudes toward those conducting it. More specifically, alienation should estrange citizens from the electoral process; alienated citizens should feel excluded from electoral politics because neither party aspires to represent their preferences in government. This should generate feelings of process-oriented distrust toward government. Alienation should isolate citizens from the day-to-day policymaking process as well; it is hard to trust government when you feel that neither party running it represents your interests (Downs 1957; Zipp 1985; Dionne 1991; Shea 2003). The farther citizens see themselves from a party, the more estranged from the political process they should feel. On the other hand, those close to a party should be more likely to trust government since they are represented in elections and have a consistent voice in government. Unlike alienated citizens, even if their party is not in full control of government institutions, their party's voice is still heard and affects policy through constitutionally designed checks and balances and minority party rights in Congress. Thus, the above reasoning allows me to specify my *alienation hypothesis*: all else equal, as a citizen's ideological distance from the closest party increases, trust should decrease. Since I am analyzing individual-level attitudes, I expect alienation to affect trust more strongly than polarization since alienation directly relates a citizen's ideology to the parties' whereas perceived polarization does not take a citizen's self-placement into consideration.

### **Models, Data, and Measurement**

My empirical analysis aims to determine whether perceived polarization and ideological alienation are meaningful predictors of trust. My theory leads me to anticipate a negative relationship between alienation and trust. All else equal, as alienation increases, trust should

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1997). However, this paper employs an individual-level analysis which does not assume any particular pattern of aggregate over-time change in polarization or alienation.

decrease. My theory also predicts a relationship (either negative or positive) between trust and polarization. If the data support the alienation and the negative polarization hypotheses, I can compare the magnitude of each effect on trust to determine which matters most.

I employ two statistical models. The first is an ordinary least squares (OLS) regression of the ANES-constructed 100-point trust composite variable on alienation, polarization, presidential and congressional approval, economic prospections and retrospections, partisan strength, ideology, and several other control variables. The second model is a logistic regression of a binary trust measure on the same variables. The models are specified in equation form as follows for each individual  $i$ :

$$\begin{aligned} \text{Trust}_i = & \beta_0 + \beta_1 \text{Polarization}_i + \beta_2 \text{Alienation}_i + \beta_3 \text{Strength of Partisanship}_i + \\ & \beta_4 \text{Ideological Self-Placement}_i + \beta_5 \text{Presidential Approval}_i + \beta_6 \text{Congressional Approval}_i + \\ & \beta_7 \text{Economic Retrospections}_i + \beta_8 \text{Economic Prospections}_i + \beta_9 \text{Controls}_i + \varepsilon_i \end{aligned}$$

I analyze data pooled from the cumulative ANES data file. Since congressional and economic performance measures begin in 1980, I am limited to this year as my starting point in pooling the data. However, I am able to include every subsequent ANES survey with the exception of 2002 -- a year in which the perceived ideology question disappeared, only to return in 2004. There are approximately 8,900 observations for each model. I weight the data using the ANES-provided post-stratification weights to ensure my sample represents the U.S. population as accurately as possible.

In my first analysis, I use the ANES-constructed trust index as my dependent variable. This measure represents a composite of four different ANES measures, each of which is mapped onto a 100-point scale, summed, and then divided by the number of valid responses.

This result is then rounded to derive each respondent's trust value.<sup>5</sup> These measures ask for respondents' assessments of government wastefulness, corruption, and responsiveness, along with an ANES trust item asking respondents how often they can trust government to "do what is right." The index is scaled from 0 for "least trusting" to 100 for "most trusting" with a sample mean of 32 (see Appendix A, Table A for descriptive statistics). In Figure 1, I graph the sample means across survey years to give the reader a visual feel for the data. Since the dependent variable approximates an interval scale, I estimate this model using OLS regression.

For my second analysis, I use an ANES survey question that was a component of the composite variable used in my first analysis, but the only one that directly asks the respondent how much he or she trusts government. Generally, political scientists regard this question as the best measure of trust available (see Owen and Dennis 2001; Gershtenson 2005). I do this in order ensure my results are robust across another prominent trust measure. The variable asks respondents how often they can "trust the government in Washington to do what is right." The response categories are scored 1 for "none of the time" (a volunteered response), 2 for "some of the time," 3 for "most of the time," and 4 for "just about always." Since 96 percent of the cases occur in the two middle categories, I collapse the four categories to form a dichotomous variable with a mean of 0.35 (see Appendix A, Table A for descriptive statistics). The sample means across survey years are graphed in Figure 2. Since this variable represents a binary outcome, I use logistic regression to estimate the model.

My predictors of interest are *perceived polarization* and *ideological alienation*. My analysis is not concerned with the objectively "correct" placement of the parties. Rather, citizens' perceptions of party ideological locations are of substantive interest because perceptions should drive individual-level trust attitudes. In other words, polarization cannot affect a citizen's trust in

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<sup>5</sup> Cronbach's alpha for the scale is 0.68 which indicates minimally acceptable internal consistency. I include the second trust model to overcome any questions about whether my results are robust across trust measures.

government if that citizen does not perceive it; citizens form attitudes through a perceptual filter. Since the ANES does not measure perceived polarization or ideological alienation directly, I derive them from measured variables: Beginning in 1980, the ANES asks respondents for ideological placements of both major political parties. Respondents can place the Republican and Democratic parties on a seven-point scale ranging from “extremely liberal” to “extremely conservative.” Respondents also place themselves on the same ideological scale. I use these responses to construct my measures of alienation and polarization. My perceived polarization measure is the absolute difference between each respondent’s placements of the parties on the seven-point ideology scale:

$$\textit{Perceived Polarization}_i = |R_i - D_i|$$

where *Perceived Polarization<sub>i</sub>* represents perceived polarization for each individual *i*, *R<sub>i</sub>* is the respondent’s perceived ideological position of the Republican Party and *D<sub>i</sub>* is the perceived ideological position of the Democratic Party. For example, a respondent who places the Democratic Party at “1” (extremely liberal) and the Republican Party at “6” (somewhat conservative) would receive a value of “5” on the perceived polarization scale ( $5 = |6 - 1|$ ). The maximum perceived polarization value is 6 (if the respondent sees the parties as polar opposites) and the minimum is 0 (if the respondent sees the parties as ideologically equivalent). Appendix, Table A displays the descriptive statistics for perceived polarization. Figure 3 displays its frequency distribution, which shows that most respondents place the parties between two and four ideological units apart. On average, citizens see the parties as 2.79 ideological units apart, with a standard deviation of 1.44 units.

I measure ideological alienation as the distance between the respondent's ideological self-placement and his or her closest party placement. More specifically, this is the minimum absolute difference between the respondent's self-placement and a party placement. It is specified in equation form as follows:

$$\textit{Ideological Alienation}_i = \min(|x_i - R_i|, |x_i - D_i|)$$

where *Ideological Alienation<sub>i</sub>* represents ideological alienation for each individual *i*, *x<sub>i</sub>* is respondent *i*'s ideological self-placement, *R<sub>i</sub>* is the respondent's ideological placement of the Republican Party, and *D<sub>i</sub>* is the respondent's ideological placement of the Democratic Party. The respondent was party indifferent (equidistant from each party) in roughly one-fifth of the cases. The sample mean of alienation is 0.85 with a standard deviation of 0.80 (see Appendix A, Table A for descriptive statistics). Figure 4 displays the frequency distribution for ideological alienation, which shows that most respondents place the closest party at or within one ideological unit of themselves. Only 51 respondents placed the closest party more than three ideological units away.<sup>6</sup> Most respondents placed the other party two, three, or four units away from themselves. There were only 13 cases in which the respondent's self-placement and *both* party placements were identical, meaning perceived polarization and ideological alienation both equal zero.

So the reader can get a feel for the macro-level data, I display the biennial survey means since 1972 for perceived polarization and ideological alienation in Figure 5. Notice that ideological alienation has remained relatively stable since 1972 whereas perceived polarization has increased significantly -- by roughly one full ideological unit or 69.4% of perceived

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<sup>6</sup> Omitting these cases has no significant effect on the regression results (see "Results" section).



polarization's standard deviation. Assuming that the parties in government and as organizations have polarized, this suggests that citizens are moving to the poles with their parties. Clearly, these trends deserve future macro-level investigation.

I model several other substantive predictors of trust. First, presidential approval is measured on a four-point scale ranging from 1 for “strongly disapprove” to 4 for “strongly approve.” Congressional approval is measured as a binary “approve/disapprove” variable. In both cases, higher approval should predict higher trust. Since we know economic evaluations affect trust, I include the ANES's five-category measure of economic retrospections, which asks respondents to assess the performance of the economy over the past year. The response categories range from 1 for “much worse” to 5 for “much better.” Additionally, I include a three-category measure of economic prospectations scored 1 for “worse,” 2 for “same,” and 3 for “better.” Rosier economic forecasts and recollections should positively influence trust.

I also control for strength of partisanship and ideological self-placement. Past research shows that party identification is not a significant predictor of trust – on average, Democrats do not tend to trust government more than Republicans or vice-a-versa (Keele 2005). However, there is evidence that independents trust government less on average than their partisan counterparts. Therefore, I model partisanship in terms of strength instead of direction by collapsing the ANES party identification scale to 1 for strong partisans, 2 for weak partisans, 3 for partisan-leaning independents, and 4 for pure independents (non-party-leaning). In terms of ideological self-placement, I expect liberals to be more trusting of government, all else equal, than conservatives, given liberals' preference for an active government.<sup>7</sup>

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<sup>7</sup> Some scholars argue that ideology measures are biased because many citizens call themselves conservative but are operationally liberal (see Stimson 2004). I constructed an alternative ideology measure based on respondents' preferences for government job guarantees similar to Keele (2005). The coefficient was significant in the predicted direction but the model fit the data more poorly than the model estimated in this paper. Therefore, I used the original ideology measure in my models.

In order to account for demographic influences on trust, I include controls for race (1 for black, 0 for white), gender (1 for female, 0 for male), region (1 for South, 0 for non-South), income (quintiles), age (in years), and education level (7-point scale). Based on a large body of prior research, we expect these demographic variables to matter in predictable ways.<sup>8</sup> For example, we would expect respondents from the South to trust the federal government less than respondents from the non-South given the South's historical resistance to federal intervention in the states.

Since I am analyzing pooled cross-sectional data, I anticipate and find post-estimation evidence of heteroscedastic residuals. I correct for them using robust standard errors. Also, to model changes in the mean of the dependent variable across survey years, I employ year dummies (excluded from the tables to save space but reported in Appendix A, Table B). Both models are identical with the exception of the estimation method (OLS vs. maximum likelihood) and the measurement of the dependent variable.

## Results

The results of the OLS regression analysis displayed in the left-hand column of Table 1 strongly support the negative polarization hypothesis and the alienation hypothesis -- each variable has a significant negative relationship with the composite trust measure. For every one-unit increase in perceived ideological distance between the parties (perceived polarization), trust decreases by 0.51 points on the 100-point scale, all else equal ( $p < 0.01$ ). Likewise, a one-unit increase in ideological distance to the closest party (ideological alienation) lowers trust on

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<sup>8</sup> I omit self-reported unemployment from the models because though King (1997) finds it to be a significant predictor of trust, it is statistically insignificant ( $p = 0.89$ ) when included in my models. I suspect this is because, unlike my models, King (1997)'s model does not control for national economic perceptions.

average by 1.04 points on the same scale ( $p < 0.01$ ). Though the alienation coefficient is larger than the polarization coefficient, the difference between them does not reach statistical significance ( $p = 0.11$ , two-tailed). Since a one-unit increase in either polarization or alienation may not be realistic given their distributions, we can add further interpretive clarity by putting these effects in the context of their standard deviations: A one standard deviation increase in polarization (1.44 ideological units) reduces trust by 0.73 points whereas a one standard deviation increase in alienation (0.80 ideological units) reduces trust by 0.83 points. Thus, both perceived polarization and ideological alienation have a similar statistically significant negative effect on trust.

The other predictors in the OLS model behave as expected.<sup>9</sup> Consistent with the established literature, congressional approval ( $\beta = 9.69$ ,  $p < 0.01$ ) and presidential approval ( $\beta = 3.09$ ,  $p < 0.01$ ) are the most powerful predictors of trust. Further, as expected, partisans tend to trust government more than non-partisans and conservatives tend to trust government less than liberals, all else equal. Economic evaluations play a large role in explaining trust as well: Those who felt the economy had improved over the past year and felt it was going to get better in the future trusted government more than those who were less rosy about the past economy and less optimistic about the future economy.<sup>10</sup>

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<sup>9</sup> Hetherington's (1998) work suggests possible endogeneity concerns with my models. As with any individual-level model, causality may flow from my dependent variable (trust) to the predictors. However, my predictors of interest -- perceived polarization and ideological alienation -- both make more theoretical sense causally preceding trust than being caused by it; in other words, it is difficult to make a compelling theoretical case that one's trust in government should cause ideological placements of the parties and one's ideological self-placement. Another possibility is that a general trust attitude causes perceived polarization, ideological alienation, and trust in government, rendering the apparent causal relationship between polarization/alienation and trust spurious. However, there is no commonly asked general trust measure in the ANES Cumulative Data File with which to test for spurious causation.

<sup>10</sup> Several control variables reached statistical significance in predicted directions as well, though I refrain from discussing them here due to space considerations. To assess possible multicollinearity among the predictors, I compute the variance inflation factor (VIF) for each. VIF values for all predictors are well below the threshold for multicollinearity concerns. Testing for influential data points among perceived polarization and ideological alienation data finds no significant outlier influence on the estimates.

To strengthen the robustness of my results, I test a similar model using the more focused dichotomous measure of trust described above. The results of this maximum likelihood (ML) estimation are displayed in the right-hand portion of Table 1. They are similar to those obtained using the 100-point trust index as the dependent variable. Though the magnitude of logistic regression coefficients cannot be interpreted directly, we can use them to assess the direction and significance of relationships. Table 1 shows that perceived polarization and ideological alienation each retain their anticipated sign and significance ( $p < 0.01$ ). Therefore, based on the two models' reinforcing results, I can conclude with confidence that the likelihood of a citizen trusting government decreases as perceived polarization and ideological alienation increase. Measures of overall model fit indicate a reasonably well-specified model: The McKelvey and Zavonia Pseudo- $R^2$  is 0.17 and the model classifies 69.4% of the cases correctly.

I can further interpret the logistic regression results by looking at predicted probabilities of trust at each value of polarization and alienation.<sup>11</sup> Table 2 shows the predicted probabilities of a trust response across values of polarization and alienation with all other predictors held at their means, as well as the 95% confidence interval for each probability. Figures 6 and 7 display the same information graphically, with 95% confidence bounds plotted as dotted lines on either side of the predicted probability line. For perceived polarization, those placing the parties at the same place in ideological space (perceived polarization = 0) have the highest predicted probability (0.35) of trusting the government, all else equal. Those with a polarization value of 4 have a predicted probability of trust that is 0.04 lower (0.31). The predicted probability of trust declines monotonically across perceived polarization values – 0.01 per unit increase in polarization – such that a minimum to maximum change in polarization reduces the predicted probability of trust by 0.06.

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<sup>11</sup> To compute and analyze predicted probabilities, I use the **prvalue** command from the **SPost** add-on to Stata written by J. Scott Long and Jeremy Freese. For more information about this add-on, see Long & Freese (2005).

For ideological alienation, the results are more dramatic: Someone whose ideological self-placement matches their placement of a political party (alienation = 0) has a 0.35 predicted probability of trusting the government, all else equal. However, when alienation is 3, the probability of trust drops by 0.07 to 0.28. Again, this relationship is monotonically decreasing, with the predicted probability of trust declining by 0.01, 0.02, or 0.03 per unit increase in alienation. Taken together, these results suggest meaningful negative impacts on trust as polarization and alienation values increase.

Finally, it would be helpful to compare the magnitude of perceived polarization and ideological alienation effects on trust with that of other known trust predictors. To do so, I compute discrete changes in the predicted probability of trust for each statistically significant ML model predictor reported in Table 1. Table 3 shows the change in predicted probability of trust after a one standard deviation increase in each predictor's value, centered about its mean and with all other variables held at their respective means.<sup>12</sup> A one standard deviation increase in perceived polarization and ideological alienation decreases the predicted probability of trust by 0.014 and 0.019, respectively. The discrete effects of polarization and alienation on trust are similar in absolute magnitude to strength of partisanship (0.019), ideology (-0.018), economic retrospections (0.020), and age (0.012). Thus, the explanatory power of perceived polarization and ideological alienation is comparable to that of several established trust predictors, though not as powerful as presidential and congressional approval.

To recapitulate, my results show that perceived polarization and ideological alienation represent two significant process-oriented predictors of trust that have been studied largely in

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<sup>12</sup> To analyze change in discrete predicted probabilities, I use the **prchange** command from the **SPost** add-on to Stata written by J. Scott Long and Jeremy Freese. For more information about this add-on, see Long & Freese (2005). I avoid assessing minimum to maximum effects since the upper categories of alienation contain few observations; I do not want to overstate the effects.

isolation from the trust literature until this point. When voters feel ideologically alienated by the political parties, they tend to trust government significantly less than they would otherwise. Perceived ideological distance between the parties is also a significant predictor of trust. Therefore, the data strongly support both the negative polarization hypothesis and the alienation hypothesis.

## **Discussion**

It was been several decades since Miller and Citrin debated the significance of political trust. Empirically, Citrin appears to have gotten the better of Miller: Performance-oriented explanations have dominated the scholarly understanding of what influences trust. So, it would appear trust is not a big deal since electoral replacement can easily fix poorly performing government leaders. However, this paper's results give Miller's side of the debate hope: When controlling for performance, the process-oriented variables perceived polarization and ideological alienation matter. Unlike ephemeral performance-oriented variables like presidential approval and economic retrospections, polarization and alienation both reflect enduring institutional characteristics, namely how the parties relate to each other and to citizens in ideological space; polarization and alienation cannot be changed quickly or easily through electoral replacement.

Polarization and alienation matter to citizens because they capture how well the parties reflect their preferences (alienation) and how effectively the parties in government operate (polarization). Citizens who perceive ideologically distant parties may trust government less than others because they fear a broken political discourse, an inability to compromise on policy decisions, and a lack of legislative productivity. Citizens who find themselves ideologically distant from the parties may trust government less than others because they feel alienated from

the electoral process, policy debates, and potential policy outcomes. Though polarization and alienation are meaningful process-oriented individual-level predictors of trust, there are likely others yet to be identified. It could end up being the case that both Miller and Citrin are right if new process-oriented variables emerge to balance the performance-oriented factors that currently dominate the menu of trust predictors.

My results suggest that divisive inter-party conflict matters when it comes to explaining trust. Political observers' critical claims that elite polarization creates a divisive political culture that turns citizens off to government are well-founded. However, trust is also affected negatively by a phenomenon that has received far less scholarly attention: alienation -- the ideological chasm between citizens and parties that do not represent their preferences (see Hibbing and Theiss-Morse 1995; 2001). In short, Downs (1957) is right: ideological proximity matters; citizens want parties that reflect their preferences. When citizens' preferences become distant from those who represent them in government, they react by trusting government less to "do what is right." Future research should tap the potential explanatory power of polarization and alienation for understanding other barometers of democratic health such as political efficacy and party identification rates.

It is important to emphasize that since I analyze individual-level relationships across time, my results are not time-bound -- in other words, they do not depend on macro-level conditions for their validity. For example, while individual-level alienation may be higher on average when the parties in government are polarized (given a citizenry with normally distributed ideological preferences), the relationships described in this paper exist independent of macro-level polarization.<sup>13</sup> In fact, alienation can be likely even when the parties are not polarized. For

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<sup>13</sup> Polarization will often lead to an alienated citizenry, but it need not always do so. For example, if the citizenry is polarized, polarizing parties will decrease alienation on average. A macro-level investigation of this topic would be helpful in understanding the dynamic relationship between polarization and alienation over time.

example, Miller (1974a; 1974b) wrote about alienation at a time when both parties were perceived as centrist and out of touch with a comparatively polarized public.

Though individual-level trust work remains interesting, the long-term consequences of process-oriented declines in trust may be best approached from a macro-level perspective. Keele's (2007) work on social capital demonstrates that dynamic causal linkages can help clarify our understanding of phenomena like trust. Like social capital, process-oriented effects like polarization and alienation have serious and deep-reaching implications for American democracy: The absence of trust may create a political climate where elites are unable to avoid gridlock. Schattschneider (1960) worried that strong (i.e., polarized) parties might alienate voters and, in turn, decrease trust. Polarization- and alienation-induced trust declines might in turn trigger mass disengagement from the political process. These defections (especially by moderates) could lead to politics dominated by ideologues who initiate a downward spiral by alienating more and more members of the citizenry until trust reaches levels that threaten regime stability.

To effectively address Schattschneider's concerns, scholars must better understand how elites and the mass public respond to one another. To the extent that elites lead the citizenry, we may observe citizens polarizing in-step with the parties, as the over-time alienation data presented in Figure 5 suggest may be happening (Shea 2003; Layman, Carsey, & Horowitz 2006). As citizens polarize, they would (on average) become less and less alienated by the closest party. Alternatively, parties may respond to lower trust by moving closer to the citizens they are alienating. If this is the case, alienation would not be as dangerous as it seems now. What ultimately happens speaks to the heart of the performance versus process debate that has informed the trust literature. If citizens and parties respond to one another, polarization and

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alienation may be relatively ephemeral, easily cured diseases. However, if the political process reinforces (and hence exacerbates) the problem, the effects could be enduring and damaging.

## Tables

**Table 1. The Effect of Perceived Polarization and Alienation on Trust, 1980-2004<sup>^</sup>**

Predictor	OLS Estimate	ML Estimate
<b>Polarization</b>	<b>-0.51</b> <b>(0.16)</b>	<b>-0.04*</b> <b>(0.02)</b>
<b>Alienation</b>	<b>-1.04*</b> <b>(0.28)</b>	<b>-0.11*</b> <b>(0.03)</b>
Strength of Partisanship	1.47* (0.25)	0.10* (0.03)
Ideological Self-Placement	-0.37* (0.16)	-0.06* (0.02)
Presidential Approval	3.09* (0.21)	0.31* (0.02)
Congressional Approval	9.69* (0.47)	0.84* (0.05)
Economic Retrospections	1.25* (0.26)	0.08* (0.03)
Economic Prospections	2.82* (0.34)	0.28* (0.04)
Age	0.01 (0.01)	0.003* (0.002)
Black (0/1)	-1.17* (0.80)	-0.37* (0.09)
Female (0/1)	-1.11* (0.44)	-0.04 (0.05)
South (0/1)	-1.12* (0.50)	0.04 (0.06)
Income (Quintile)	-0.06 (0.22)	-0.05 (0.02)
Education (1/7)	0.72* (0.15)	0.02 (0.02)
Constant	7.02* (1.69)	-2.57* (0.20)
R <sup>2</sup> /McKelvey & Zavoina's R <sup>2</sup>	0.19	0.17
$\ell$	--	-5059.46
Wald $\chi^2$	--	903.83*

**Notes:** The dependent variable in the OLS estimation is trust in government measured using the ANES trust index scaled from 0 for “least trusting” to 100 for “most trusting.” The dependent variable in the ML estimation is trust in government measured using a binary scale of 1 for “trust” and 0 for “do not trust.” Table entries are OLS/ML estimates with robust standard errors in parentheses. Year dummies in Appendix A, Table B. ANES supplied weights used to ensure data accurately represent population. \* $p < 0.05$  (two-tailed); N = 8,904 (OLS); N = 8,901 (ML). <sup>^</sup>2002 not included.

**Table 2. Predicted Probabilities of Trust by Polarization and Alienation Values, 1980-2004<sup>^</sup>**

Polarization/Alienation Value	Probability of Trust = 1 for Polarization	95% Confidence Interval	Probability of Trust = 1 for Alienation	95% Confidence Interval
0	0.35	0.33-0.38	0.35	0.33-0.36
1	0.34	0.32-0.36	0.32	0.31-0.33
2	0.33	0.32-0.35	0.30	0.28-0.32
3	0.32	0.31-0.33	0.28	0.25-0.30
4	0.31	0.30-0.33	0.25	0.22-0.29
5	0.30	0.28-0.32	0.23	0.19-0.28
6	0.29	0.27-0.32	0.22	0.16-0.27

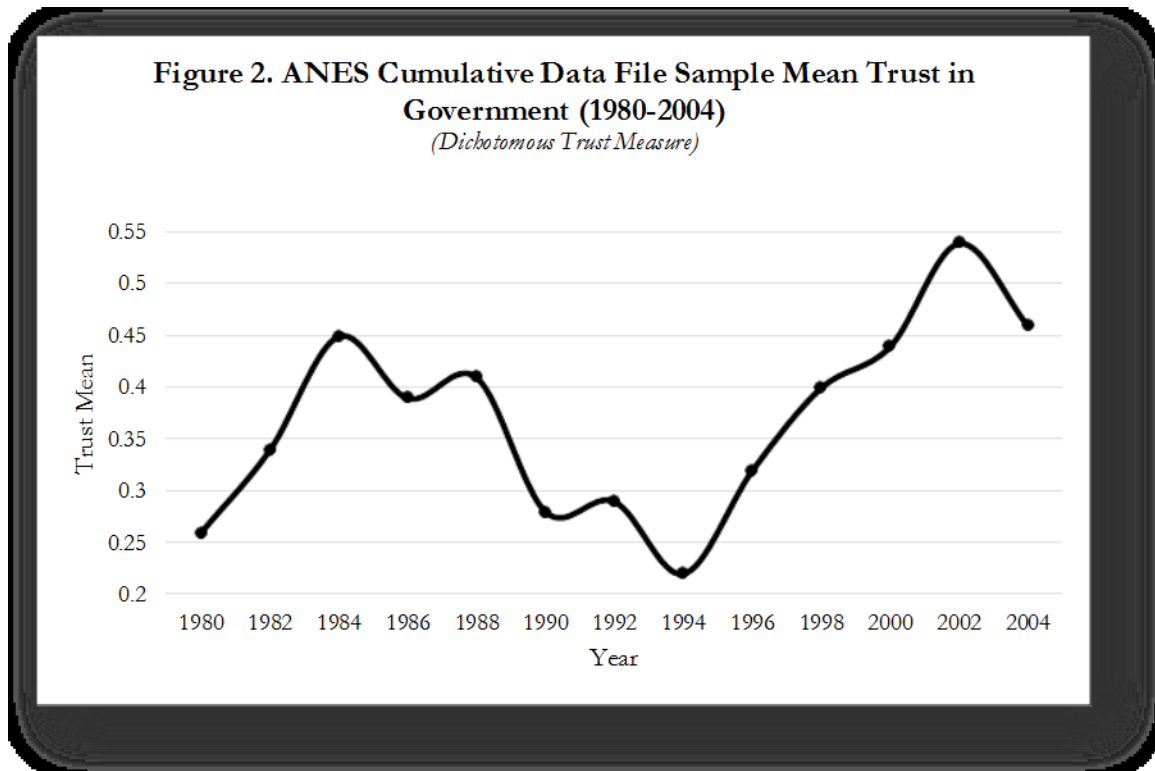
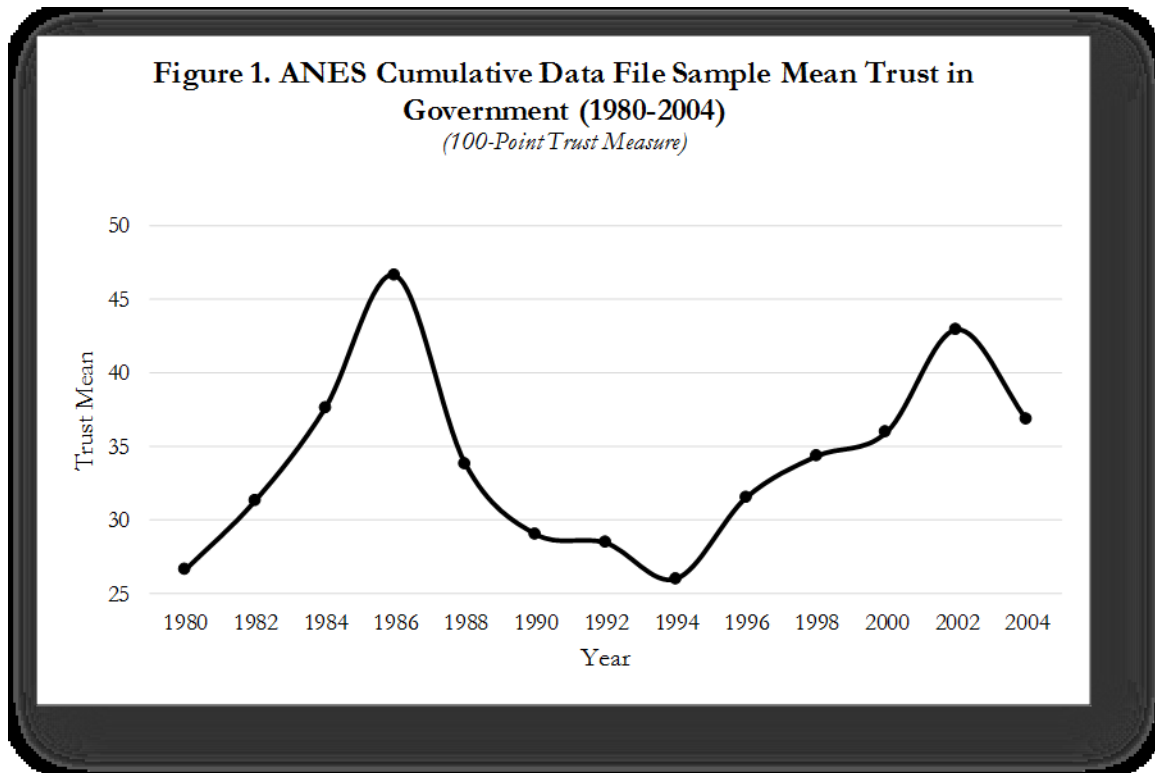
**Notes:** Predicted probabilities calculated using the **prvalue** command from the **SPost** add-on package for Stata while holding all predictors at their mean (Long & Freese 2005). Predicted probabilities are based on ML estimation results in Table 1. <sup>^</sup>2002 not included.

**Table 3. Discrete Change in Predicted Probability of Trust, 1980-2004<sup>^</sup>**

Predictor	$\bar{x} \pm (s.d.* 0.5)$
<b>Polarization</b>	<b>-0.014</b>
<b>Alienation</b>	<b>-0.019</b>
Strength of Partisanship	0.019
Ideological Self-Placement	-0.018
Presidential Approval	0.080
Congressional Approval	0.090
Economic Retrospections	0.020
Economic Prospections	0.044
Age	0.012
Black (0/1)	-0.024

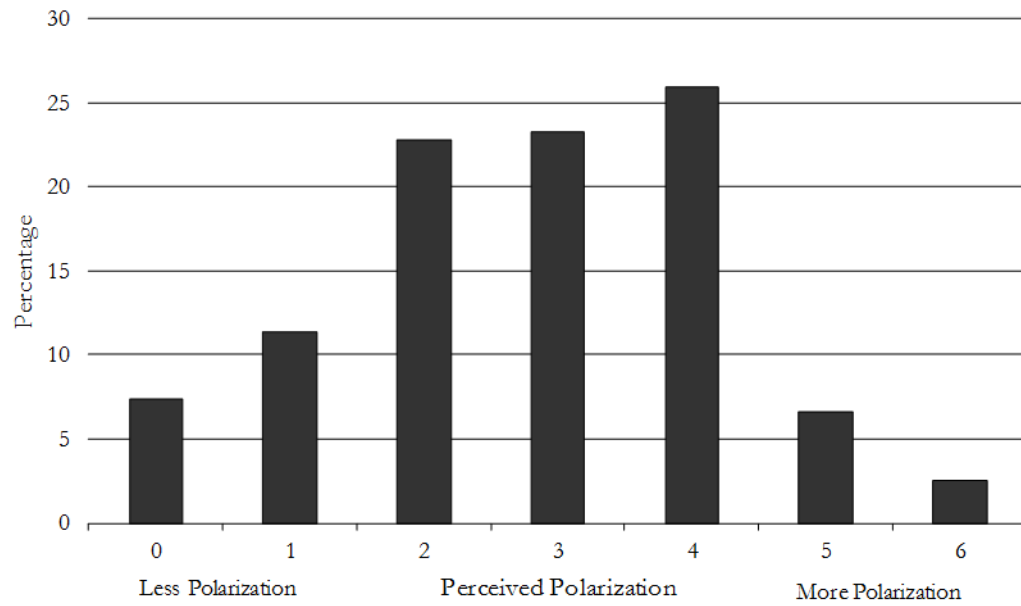
**Notes:** Only statistically significant predictors included. To analyze change in discrete predicted probabilities, I use the **prchange** command from the **SPost** add-on package for Stata while holding all predictors at their mean (Long & Freese 2005). Predicted probabilities are based on ML estimation results in Table 1. <sup>^</sup>2002 not included.

## Figures



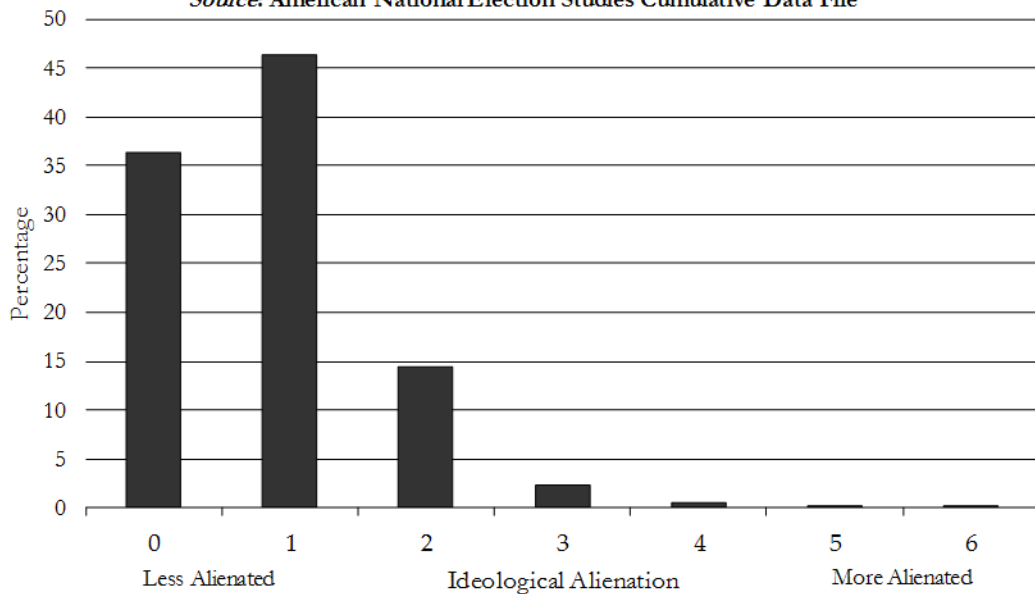
**Figure 3. Frequency Distribution of Perceived Polarization**

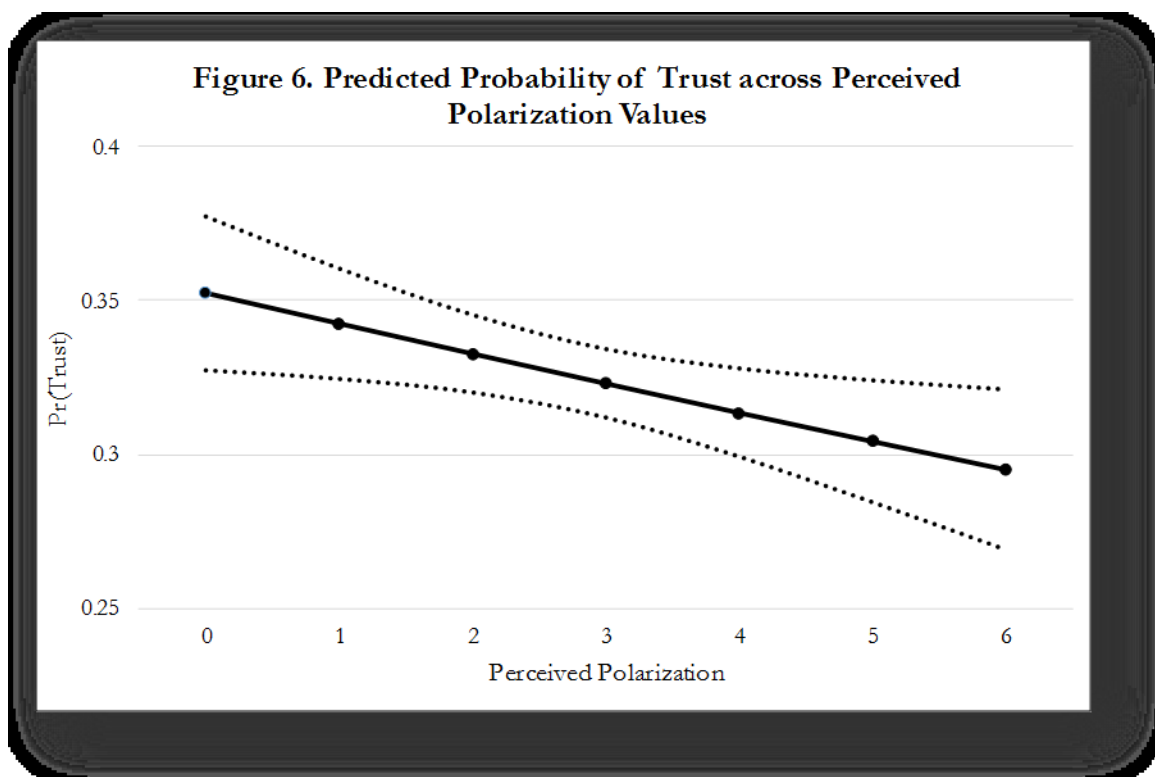
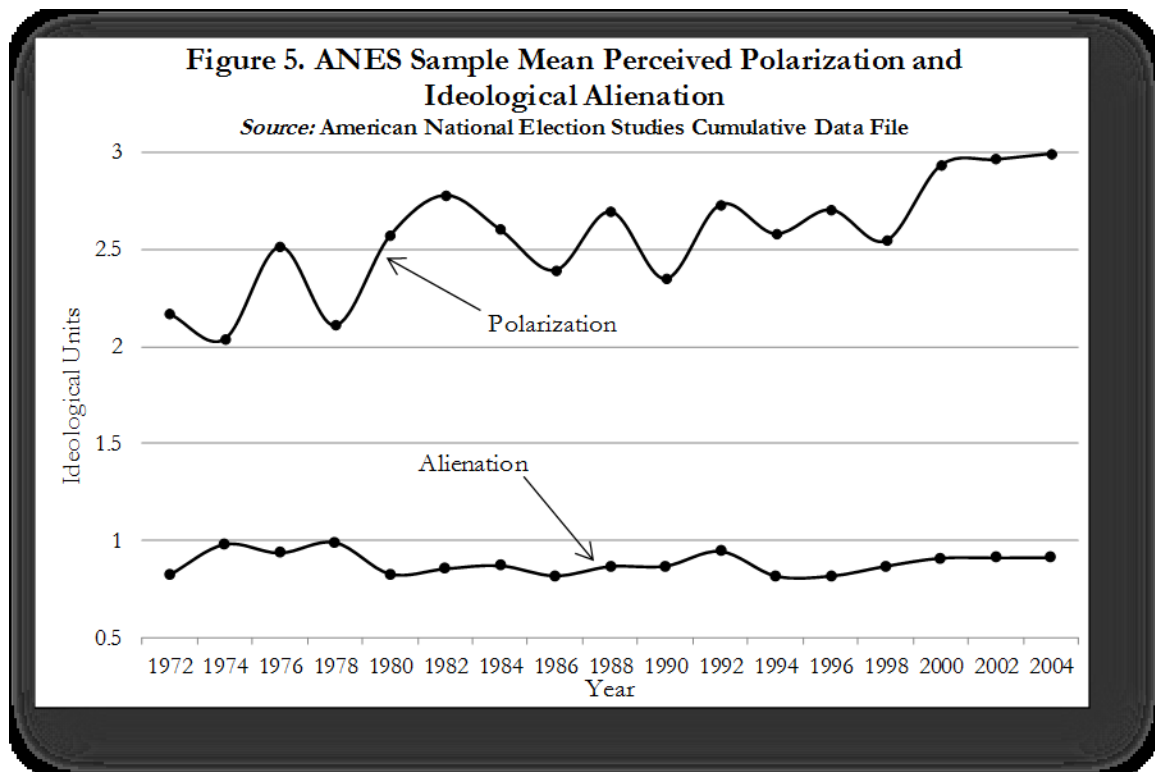
*Source: American National Election Study Cumulative Data File*

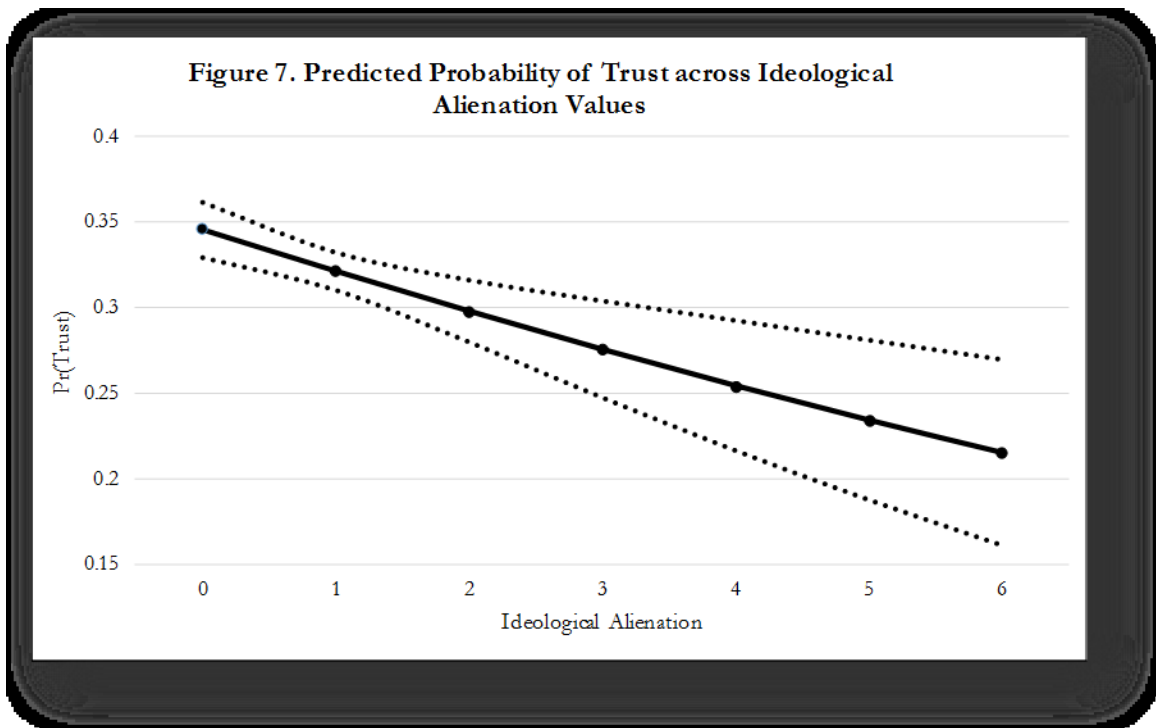


**Figure 4. Frequency Distribution of Ideological Alienation**

*Source: American National Election Studies Cumulative Data File*







## II. LEFT BEHIND: COMPETITIVE NOMINATIONS AND COMPARATIVE CANDIDATE AMBIVALENCE

The 2008 presidential election featured the most competitive Democratic Party nomination contest since 1984. Both frontrunners, New York Senator Hillary Clinton and Illinois Senator Barack Obama, presented strong candidacies: Clinton attracted key traditional Democratic constituencies (working-class whites, seniors, Hispanics, and women) and touted her experience advantage over Obama, a relative newcomer to the national political scene. Obama's support base in the primaries consisted of young (under 45 years old), well-educated, affluent, and black voters. Throughout the primary and caucus season, prominent Democrats worried about facing their first brokered convention since 1984.<sup>14</sup> Clinton claimed an overall popular vote lead, winning primaries in several delegate-rich states including California, New York, and Pennsylvania.<sup>15</sup> By the time the final primaries ended in June, Clinton had amassed 1,640 pledged delegates to Obama's 1,763 and 286 superdelegates to Obama's 395. Obama lacked enough pledged delegates to clinch the nomination outright; it was only after a last-minute surge in superdelegate commitments that he surpassed the required 2,118 delegates to secure the nomination.<sup>16</sup> Clinton officially withdrew from the race on June 7. Democrats found themselves consolidating support for their nominee against an opponent with a four month head start.

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<sup>14</sup> For example, see Van Natta & Becker (2008).

<sup>15</sup> See Healy (2008). Clinton's claim was criticized as inaccurate because her tally relied on primary results from non-party sanctioned primaries in Michigan and Florida. Obama was not on the ballot in the Michigan primary.

<sup>16</sup> See Zeleny (2008).



Competitive nominations like the Democrats experienced in 2008 are purported to weaken a party and its nominees by: (1) causing intra-party conflict that divides the party into factions; (2) exposing the eventual nominee's weaknesses that the opposing party can exploit; (3) diverting resources from the general election to the nomination campaign; and (4) causing disaffected supporters of nomination losers (including activists) to demobilize, defect, or abstain from voting for the party nominee on Election Day due to "sour grapes" (see Key 1958; White 1961; Ranney 1975; Comer 1976; Kirkpatrick 1976; Sullivan 1977-1978; Ware 1979; Polsby & Wildavsky 1980; Wayne 1981; Polsby 1983). Political scientists have extensively studied the effects of so-called "divisive" nominations on general election behavior across gubernatorial, senatorial, and presidential races, using mostly aggregate (state-level) data.<sup>17</sup> Some studies find that such primaries have negative "carryover effects" on the general election campaign (Bernstein 1977; Born 1981; Kenney & Rice 1984; Southwell 1986; Stone 1986; Kenney & Rice 1987; Lingle, Owen, & Sonner 1995), whereas others find they have insignificant or mixed effects (Hacker 1965; Pierson & Smith 1975; Born 1981; Buell 1986; Kenney & Rice 1987; Atkeson 1998) or even positive ones (Stone, Atkeson, & Rapoport 1992; Atkeson 1993; Hogan 2003; Shafer & Wichowsky 2009).

Most competitive nomination studies that find negative aggregate-level effects loosely attribute them to unmeasured individual-level disaffection among thwarted primary voters toward their party for failing to nominate their preferred candidate. These "sour grapes" are typically assumed to mediate the causal linkage between nomination competitiveness and state-level general election results. As Ware (1979) points out, this disconnect between individual-level

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<sup>17</sup> "Divisive primary" analyses typically operationalize divisiveness as the competitiveness of the primary (i.e., difference in primary vote share between candidates). However, as Wichowsky & Niebler (2010) point out, the 2008 Democratic nomination campaign illustrates that a competitive nomination is not necessarily divisive (though competitiveness is almost certainly a necessary condition for divisiveness). I suspect that "divisiveness" is a function of the level of ideological and policy fissure between the nomination candidates.

theory and aggregate-level data makes any causal interpretation tenuous. Individual-level analyses have attracted far less scholarly attention (but see Lingle 1980; Southwell 1986; Stone 1986; Kenney & Rice 1988; Pierce 2003).<sup>18</sup> Notable recent work by Henderson, Hillygus, & Tompson (2010) finds that “sour grapes” did not increase the probability of McCain defection among Democratic primary voters in 2008. However, the authors operationalize “sour grapes” as a non-specific “frustrated” response on a checklist of feelings about the upcoming presidential election instead of something nomination-specific (524).<sup>19</sup> The preponderance of both state- and individual-level competitive nomination studies focus on vote choice impacts, potentially missing less obvious but equally important impacts on thwarted voters’ attitudes that occur prior to Election Day and may provide a necessary causal link to their voting behavior.

Surprisingly, no studies to date have considered potential competitive nomination carryover effects on the attitudes of thwarted voters. In this paper, I argue that thwarted voters may be susceptible to increased levels of attitudinal ambivalence toward the general election candidates after their preferred candidate (the competitive nomination loser) withdraws from the election. Attitudinal ambivalence is not a normal attitude structure – it is an inherently unstable state of internal attitude conflict. Attitudinal ambivalence, which has been developed and studied in the psychology literature, can be broadly understood as “an individual’s endorsement of competing considerations relevant to evaluating an attitude object” (Lavine 2001, 915). In a political context, ambivalence occurs when (1) an individual simultaneously holds positive and negative attitudes toward the same candidate, party, or policy; or (2) an individual holds positive attitudes toward competing candidates, parties, or policies (Lavine 2001; Basinger & Lavine

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<sup>18</sup> Two of these analyses employ simultaneous equation models in which nomination candidate preference indirectly affects vote choice (Kenney & Rice 1988) and participation (Stone 1986) through candidate evaluations. Others look at turnout effects (Southwell 1986).

<sup>19</sup> Further, the authors fail to purge strategic primary voters from the sample, though they acknowledge their presence in it.

2005). In order to signify ambivalence, the competing attitudes must be similar in magnitude and of at least moderate intensity. Thus, weakly held but conflicting beliefs do not generate sufficient conflict to denote ambivalence (Mowrer 1960; Thompson, Zanna, & Griffin 1995; Lavine 2001).

Ambivalence is important because it has meaningful implications for how citizens think and behave. It can paralyze a voter's decision-making process, delaying formation of voting intentions and diminish the impact of these intentions on vote choice (Lavine 2001; Alvarez & Brehm 2002). Ambivalence makes political attitudes more moderate, less stable, and less accessible (Huckfeldt & Sprague 2000; Lavine 2001; Meffert, Gue, & Lodge 2004; Lodge & Taber 2005) and partisan attachments and policy judgments more variable (Alvarez & Brehm 2002; Rudolph 2005; Keele & Wolak 2006). Ambivalence affects the accuracy and processing of candidate evaluations, as well as how issues inform them (Lavine 2001; McGraw, Hasecke, and Conger 2003; Meffert, Gue, & Lodge 2004; Basinger & Lavine 2005; Craig et al. 2005). These effects may result in poorly reasoned voting decisions where the voter ignores competing considerations in an effort to avoid the discomfort they cause, instead relying on cues or gut feelings. Ambivalence may also discourage political participation (Mutz 2002). However, ambivalence also tends to make citizens more deliberative, receptive to new messages, and even-handed decision makers than their less conflicted counterparts (Sniderman 1981; Gue & Meffert 1998; Green, Visser, & Tetlock 2000). Rather than ignorance, ambivalence suggests sophisticated multi-dimensional political thinking.

To date, scholars have largely treated ambivalence as an exogenous variable, focusing on its consequences and magnitude as it pertains to issues (e.g., Craig, Kane, & Martinez 2002), institutions (e.g., McGraw & Bartels 2005), candidate evaluation (e.g., McGraw, Hasecke, & Conger 2003; Meffert, Gue, & Lodge 2004), and partisanship (e.g., Keele & Wolak 2006). However, some recent scholarly attention has been devoted to uncovering the underlying

sources of ambivalence. This fledgling work finds that partisan and ideological strength discourage ambivalence, but exposure to diverse social messages, education, information, and need for cognition encourage it (Mutz 2002; Huckfeldt, Mendez, & Osborn 2004; Steenbergen & Brewer 2004; Rudolph & Popp 2007). Other work suggests that ambivalent attitudes result from value conflict (Alvarez & Brehm 2002; Steenbergen & Brewer 2004; Keele & Wolak 2006), affective reactions (Lavine et al. 1998), and group and candidate trait evaluations (Meffert, Guge, & Lodge 2004; Lavine & Steenbergen 2005).

Limited research has attempted to uncover sources of ambivalence during a campaign: Keele & Wolak (2008) investigate the effect of state-level campaign context on ambivalence, finding that it tends to be higher among residents of battleground states where voters are exposed to a greater quantity of conflicting political messages. Only one published paper has investigated campaign ambivalence longitudinally: Using the same dataset I use in this paper, Rudolph (2011) models ambivalence change over the course of the 2008 presidential campaign, though he only makes a first cut at decomposing the sample to examine possible between-group heterogeneity.<sup>20</sup> While he finds that ambivalence generally declines as the campaign progresses and is weaker among devout partisans than dispassionate ones, he does not investigate ambivalence as a possible nomination carryover effect.<sup>21</sup> Therefore, important work remains to be done to determine whether intra-party competition causes ambivalence among thwarted voters toward the general election candidates.

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<sup>20</sup> This was preceded by an unpublished paper (Steenbergen, Lavine, & Goolsby 2006) that used data from two ANES panels to study the stability of partisan ambivalence across election years, though the data did not permit analysis within a single campaign.

<sup>21</sup> He does attempt to examine whether Democrats' comparative candidate ambivalence toward McCain and Obama in the general election campaign was a function of their feelings toward Hillary Clinton and finds that ambivalence change among those who liked and disliked Clinton is not distinguishable. However, he restricts his analysis to Clinton "likers" instead of Clinton primary/caucus voters. The problem with this is that Clinton "likers" and Obama "likers" are not mutually exclusive categories, leaving open the possibility that people who like both Clinton and Obama will contaminate the results.

This paper assesses whether people who vote for a competitive nomination loser in their state's primary or caucus are more likely than other electoral groups (i.e., independents, other partisans, and supporters of nomination winners) to experience ambivalence during the general election campaign. Specifically, my analysis investigates *comparative candidate ambivalence*, which entails simultaneously holding positive attitudes toward both general election candidates. Under this conception of ambivalence, the more positive an individual's joint evaluation of the candidates, the higher their ambivalence level; the more negative an individual's joint evaluation of the candidates, the lower their ambivalence level; the more univalent an individual's evaluation of the candidates (positive evaluation of one candidate *relative* to the other), the lower their ambivalence level. Comparative candidate ambivalence is a possible *carryover effect* of competitive nominations on thwarted voters. A carryover effect is an impact of a competitive nomination contest that uniquely affects thwarted nomination voters' attitudes or behavior during the general election campaign. I refer to comparative candidate ambivalence in this context as *carryover ambivalence*.

My analysis attempts to determine whether there is evidence of carryover ambivalence among thwarted primary or caucus voters. Specifically, I assess whether people who report voting for a competitive nomination loser in their state's primary or caucus are more likely than other electoral groups to experience comparative candidate ambivalence during the post-nomination campaign. Thwarted voters should be more likely than other electoral groups to gather positive information about both party nominees during the general election due to (1) "sour grapes" toward their party from the nomination campaign; and (2) party-loyalty driven pressure to support their party nominee. This influx of positive information about the general election candidates should increase comparative candidate ambivalence among thwarted voters relative to other groups.

In the following sections, I formally develop my theoretical expectations. Then, I derive testable hypotheses from these expectations and test them using data from the 2008-09 American National Election Studies (ANES) Panel Study. Finally, I interpret my results, discuss their implications, and offer guidelines for future work. My findings help sharpen the scholarly understanding of competitive nomination effects as well as the causes and dynamic characteristics of ambivalence during campaigns.

### **Theoretical Expectations for Comparative Candidate Ambivalence**

This paper investigates comparative candidate ambivalence, which is characterized by simultaneously positive evaluations of the general election candidates, as a carryover effect of a competitive nomination race. Here, I discuss how motivated reasoning theory underpins my theoretical expectations for each level of analysis (cross-sectional and longitudinal) and I use these expectations to formalize testable hypotheses.

#### *Cross-Sectional Expectations*

Thwarted voters should be more likely than other electoral groups to experience comparative candidate ambivalence during the general election campaign because they are uniquely motivated to gather positive information about *both* parties' nominees. This motivation is best understood in the context of motivated reasoning theory, which argues that people are, in large part, motivated by directional goals when gathering and processing information (Kunda 1990; Lodge & Taber 2000; Redlawsk 2002; Taber & Lodge 2006). These goals cause individuals to purposely bias information gathering and processing *directionally* (positively or negatively). *Confirmation bias* entails disproportionately seeking or accepting information that is consistent

with the directional goal. *Disconfirmation bias* entails avoiding or discounting information that is inconsistent with the directional goal.

Once thwarted voters' preferred candidate loses the nomination, they should face competing directional motivations: First, they should experience residual bitterness, or "sour grapes," about the nomination outcome that motivates them to seek positive (and avoid negative) information about -- and even consider voting for -- the opposing party's candidate in the general election. Thwarted voters, who were heavily invested in the nomination loser's candidacy, should be upset at their party for choosing a different candidate. While the competitive nomination literature often attributes behavior such as defecting to the opposing party or abstaining from voting to "sour grapes" effects, my theory does not require a behavioral commitment. Instead, I only expect that thwarted voters will be more likely than other groups to gather positive information about the opposing party's candidate during the general election campaign.

Second, party loyalty pressure should cause thwarted voters to seek positive (and avoid negative) information about their party nominee. Competitive nomination battles expose citizens to large quantities of candidate information through debates, media coverage, public appearances, and advertising (especially in states that hold binding primaries). Candidate loyalty represents the salient directional goal during the nomination campaign: loyalists should discount negative information (disconfirmation bias) and accept positive information about their preferred candidate (confirmation bias), while accepting negative information and discounting positive information about opponents. After the nomination campaign ends, thwarted voters should possess a negatively biased stockpile of information about the party nominee. During the general election campaign, partisan directional goals become salient, motivating thwarted voters to gather and internalize positive information about the party nominee.

These two competing motivations considered together make thwarted voters more likely than other electoral groups to simultaneously hold positive opinions of both candidates during the general election campaign and therefore experience comparative candidate ambivalence. Since other partisans should not experience a similar change in directional motivation after the nomination campaign ends, I expect thwarted voters to have higher comparative candidate ambivalence levels than all other partisans, who should be fairly univalent in favor of their party nominee. Prior work shows that ambivalence increases as partisan strength decreases (e.g., Keele & Wolak 2008), so independents should experience more ambivalence throughout the campaign than all other partisans, including thwarted voters. The preceding theoretical development allows me to formalize my *cross-sectional ambivalence hypothesis*:

**During the general election campaign, thwarted voters should experience higher average comparative candidate ambivalence than all other electoral groups except independents.**

#### *Longitudinal Expectations*

Motivated reasoning theory also informs my theoretical expectations about how thwarted voters' comparative candidate ambivalence should change longitudinally over the course of the general election campaign relative to other electoral groups. I have no theoretical reason to expect discernible ambivalence differences between soon-to-be thwarted voters and other electoral groups during the nomination campaign since everyone should be motivated by candidate loyalty goals. However, I expect to see a greater positive change in comparative candidate ambivalence among thwarted voters than other electoral groups once the losing nomination candidate withdraws from the race. After their candidate withdraws, thwarted voters' directional goals should change from candidate loyalty to a competing mixture of "sour



grapes” backlash and party loyalty. As discussed above, “sour grapes” should increase the tendency for thwarted voters to gather positive information about the opposing party nominee whereas party loyalty-driven pressure to re-evaluate the party nominee positively should cause thwarted voters to gather positive information about their party nominee. These directional pursuits should lead to a rise in positive attitudes toward both party nominees during the general election campaign, which, by definition, directly translates to a rise in comparative candidate ambivalence.

I expect greater positive change in comparative candidate ambivalence among thwarted voters than other groups since only thwarted voters should experience a change in directional motivation when their candidate drops out of the race. Supporters of nomination winners lack “sour grapes” motivation to engage in positive information gathering about opposing candidates since their preferred candidate remains in the general election race. These voters may seek information about the opposing party’s candidate during the general election, but it should be subject to heavy disconfirmation and confirmation biases which should polarize their candidate evaluations, making them heavily one-sided, or univalent. Unlike thwarted voters, their directional goals of candidate-loyalty (during the nomination campaign) and party-loyalty (during the general election campaign) reinforce each other. Therefore, I expect nomination winner supporters’ mean ambivalence level to decrease relative to thwarted voters’ mean ambivalence level during the general election campaign.

Likewise, partisans who were attached to neither competitive nomination candidate have no “sour grapes” motivation to seek positive information about the opposing party nominee. However, they are motivated by party loyalty pressure to gather positive information about their own party nominee. The net result of their informational pursuit is the accumulation of positive information about one candidate but not the other (i.e., univalence), not ambivalence. Therefore,

I expect ambivalence to decrease among other partisans relative to thwarted voters during the general election campaign. Finally, independents are not subject to partisan directional constraints or “sour grapes” effects. Therefore, I do not expect their ambivalence level to change significantly during the general election campaign, meaning that thwarted voters’ ambivalence should increase relative to independents.

The preceding discussion allows me to generate a clear testable hypothesis about longitudinal ambivalence among thwarted voters:

**Thwarted voters’ ambivalence should increase more than (or decrease less than) all other electoral groups during the general election campaign.**

Though I expect comparative candidate ambivalence to rise among thwarted voters relative to other groups after their candidate exits the race, I add the caveat that I expect thwarted voters’ ambivalence to eventually decline in *absolute* terms over the course of the general election campaign as they digest campaign information into coherent opinions. Cognitive dissonance theory tells us that holding positive evaluations of both candidates is not cognitively sustainable for the entire campaign. Indeed, it predicts that people should quickly attempt to resolve inconsistencies in their belief structure because retaining them causes discomfort (Festinger 1957; 1964). We already know that ambivalence generally declines across all individuals during the campaign (Rudolph 2011). Therefore, I have theoretical and empirical grounds to expect that after an initial rise in comparative candidate ambivalence following withdrawal of the competitive nomination loser, thwarted voters’ ambivalence will decline somewhat as the general election approaches.

There is also a somewhat weaker theoretical case supporting the null hypothesis of no carryover ambivalence effects. First, party identification is perhaps the most powerful predictor

of political attitudes and behavior in the public opinion literature (Campbell et al. 1960). Therefore, even seemingly weak partisans may be constrained to such an extent by their partisanship that it overcomes any tendency toward ambivalence. Primary and caucus voters tend to be particularly devoted partisans, so there may be few thwarted voters in practice who are actually weak enough partisans to be susceptible to ambivalence, even if there were conditional effects on weak partisans (see Stone, Atkeson, & Rapoport 1992).<sup>22</sup> Second, if the nomination candidates have similar ideological and policy positions (like Obama and Clinton in 2008),<sup>23</sup> thwarted voters should find it fairly easy to transition their support to the nomination winner, especially given party loyalty pressure to do so.<sup>24</sup> Findings that confirm this null hypothesis would not be in vain since they would provide robust evidence against ambivalence as an individual-level competitive nomination carryover effect.

## **Data and Measures**

To test my hypotheses, I model between-group differences in comparative candidate ambivalence during the 2008 presidential campaign. As discussed above, the 2008 Democratic nomination featured intense competition between Hillary Clinton and Barack Obama, making it an ideal competitive nomination race with which to test my hypotheses. I use data from the 2008-09 ANES, which was conducted using two cohorts that participated in up to ten waves

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<sup>22</sup> Though not tested by the models I report, I test for conditional strength of partisanship effects using a separate series of models, footnoted in the “Results” section.

<sup>23</sup> Given Clinton and Obama’s perceived ideological and issue stances, Henderson, Hillygus, & Thompson (2010) are surprised to find that their ideological differences and position on the Iraq War were the most important predictors of a defection to McCain among Democratic primary voters. Though the differences best explain defection to McCain, it does not necessarily follow that these differences were particularly large in absolute terms (though perhaps Democrats perceived them as such), especially when compared with those between McCain and Obama.

<sup>24</sup> In fact, the similarity of the candidates’ ideological and policy preferences probably makes 2008 an especially conservative test of my hypotheses. Ideological and policy fissures between candidates might be responsible for causing a competitive primary to become “divisive.” In these cases, we would expect carryover ambivalence to be at its highest level.

beginning in January 2008 (cohort 1) or September 2008 (cohort 2) and ending in August 2009. I use cohort 1 data only since my analysis requires pre-September 2008 data. The survey design mitigates nomination-winner bias by recording primary or caucus vote choice in the survey wave immediately following the respondent's primary/caucus (see Atkeson 1999).

Attitude scholars have identified two theoretically necessary and sufficient conditions for ambivalence: First, positive and negative reactions to the attitude object must be of similar magnitude; dissimilar magnitude indicates univalence (attitudes concentrated at one end of the positive-negative spectrum). Second, ambivalence should be a positive function of attitude intensity; weakly held attitudes should indicate less internal conflict (e.g., Hass et al. 1991; Thompson, Zanna, & Griffin 1995). These conditions are best captured with the ambivalence index developed by Thompson, Zanna, and Griffin (1995):

$$Ambivalence_{ind} = \frac{P + N}{2} - |P - N|$$

where the *ind* subscript refers to the level of analysis (individual candidate/party), *P* captures the number of positive reactions to the candidate/party and *N* captures the number of negative reactions. The division by two indicates the average of the number of positive and negative reactions, satisfying the intensity criterion. The similarity criterion is met by the absolute value of the difference between the number of negative and positive reactions.

My dependent variable is a measure of *comparative candidate ambivalence* constructed using two survey questions that ask, “Do you like [candidate], dislike [him/her], or neither like nor dislike [him/her]?” immediately followed by, “Do you [like/dislike] [him/her] a great deal, a moderate amount, or a little?” Though these items are univalent by design and hence do not

assess ambivalence toward individual candidates as specified above, they allow me to construct a measure of *comparative* ambivalence toward McCain and Obama. I operationalize this measure using an adaptation of the above-described Griffin index first proposed by Lavine (2001) and subsequently used by Basinger & Lavine (2005), Rudolph & Popp (2007), Keele & Wolak (2008), and Rudolph (2011). Here, the index takes the form:

$$AMB_{comp} = \frac{M + O}{2} - |M - O|$$

where the *comp* subscript refers to the type of analysis (comparative between McCain and Obama), *M* captures the reaction to John McCain, and *O* captures the reaction to Barack Obama. This formula differs from the original because it compares reaction intensity and similarity *between* candidates instead of positive and negative reactions to a single candidate: Here, ambivalence increases as attitudes toward the candidates become less polarized and jointly more positive. Following Rudolph (2011), each reaction is represented by a seven-point scale of individual candidate like/dislike generated by combining the two survey items delineated above. The scale for *M* and *O* ranges from 1 (dislike a great deal) to 7 (like a great deal). The formula produces an index with a numerical range of -2 in cases of perfect univalence (e.g., *M*=7 and *O*=1) to 7 in cases of perfect ambivalence (when *M*=7 and *O*=7).<sup>25</sup> I use comparative candidate ambivalence measured during three survey waves (*AMB<sub>June</sub>*, *AMB<sub>Sept.</sub>*, and *AMB<sub>Oct.</sub>*) as dependent variables in my cross-sectional models. I use change in comparative candidate ambivalence across selected survey waves (*AMB<sub>Jan.-June</sub>*, *AMB<sub>June-Sept.</sub>*, and *AMB<sub>June-Oct.</sub>*)

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<sup>25</sup> Note that under this modified ambivalence formula, simultaneous dislike is not equivalent to simultaneous liking (Steenbergen & Brewer 2004; Rudolph 2005). Ambivalence is higher in a condition of simultaneous liking than simultaneous disliking.

as dependent variables for my three longitudinal models . I report descriptive data for my cross-sectional dependent variables in Table 4 and longitudinal dependent variables in Table 5.

My main predictors of interest are a series of dummy variables representing the following electoral groups: Obama primary/caucus voters (hereafter, Obama voters), McCain primary/caucus voters (hereafter, McCain voters), other Democratic Party identifiers, other Republican Party identifiers, and independents.<sup>26</sup> Clinton voters serve as the baseline category in order to facilitate intergroup comparisons. I exclude independent and party-bucking primary voters from the corresponding nomination candidate voter groups (i.e., Clinton, Obama, McCain) and instead classify them according to their party identity because these voters should experience ambivalence differently than those who voted for candidates from their own party, especially if they were voting strategically (perhaps to thwart a particular candidate).<sup>27</sup>

I draw the remaining predictors from the nascent literature on the sources of ambivalence (see Rudolph & Popp 2007; Keele & Wolak 2008; Rudolph 2011).<sup>28</sup> This literature suggests heterogeneous information exposure is a primary cause of ambivalence (Barker & Hansen 2005; Rudolph & Popp 2007). Therefore, I include several measures best thought to

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<sup>26</sup> Due to research suggesting that independent “leaners” (independents who feel closer to one major party than the other) tend to behave like partisans, I classify them as partisan identifiers (Keith et al. 1992). Only “pure” independents (those who feel closer to neither party) are classified as independents.

<sup>27</sup> The data suggest significant strategic voting among Republicans: 55 Republicans voted for Democrats in primaries whereas only 16 Democrats voted for Republicans. Eighteen respondents reported a primary or caucus vote choice despite having stated previously that they were sure they did not vote in the primary or caucus. These cases were placed in the partisan category corresponding to the respondent’s party identification.

<sup>28</sup> Unfortunately, there are unavoidable endogeneity issues with my research design. If a choice was available, I picked measures from the earliest wave in which it was taken so as to minimize or eliminate these issues. Unfortunately, due to the survey design, certain established ambivalence predictors are only measured in survey waves that occur after the time period I analyze (e.g., *care who wins*), thus giving me the undesirable choice between a misspecified model and one with possible endogeneity issues. I do not have reason to suspect endogeneity between my group dummies and ambivalence since primary voting and party identification measures are taken prior to ambivalence measures.

capture it: First, I use a five-point measure of *educational attainment*.<sup>29</sup> Highly educated citizens are more likely to consume a large quantity of information, think critically about it, and consider multiple viewpoints than their less educated counterparts, making them more susceptible to ambivalence. Second, following Rudolph (2011), I measure the ability to engage in effortful information processing using a seven-point scale of *political knowledge* constructed from six questions about American politics. The variable is scaled from 0 for no questions answered correctly to 6 for all questions answered correctly.<sup>30</sup> Third, I measure desire to engage in effortful processing using a *need for cognition* scale constructed from the standard ANES two-item battery.<sup>31</sup> Need for cognition has been associated with decreased reliance on informational shortcuts and using more complex cognitive processes when thinking about candidates (Cacioppo et al. 1986; Ahlerting 1987; Condra 1992; Haugtvedt, Petty, & Cacioppo 1992).

My final informational heterogeneity exposure measure is state-level *party competition*. It follows that voters in states with competitive parties should be exposed to greater quantities of competing campaign information in the form of advertising, media coverage, and politically diverse social interactions than voters in single-party dominated states. Therefore, they should be more ambivalent, on average, than those in less competitive states. Following Rudolph (2011), I

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<sup>29</sup> The 2008-09 ANES Panel Study derives an overall educational attainment variable that categorizes respondents as follows: (1) no high school diploma; (2) high school diploma; (3) some college; (4) bachelor's degree; and (5) graduate degree.

<sup>30</sup> I use the following wave 2 questions: (1) Do you happen to know how many times an individual can be elected President of the United States under current laws? (2) For how many years is a United States Senator elected – that is, how many years are there in one full term of office for a U.S. Senator? (3) How many U.S. Senators are there from each state? (4) For how many years is a member of the United States House of Representatives elected – that is, how many years are there in one full term for a U.S. House member? (5) According to federal law, if the President of the United States dies, is no longer willing or able to serve, or is removed from office by Congress, the Vice President would become the President. If the Vice President were unable or unwilling to serve, who would be eligible to become president next? (6) What percentage of the vote of the House and Senate is needed to override a Presidential veto?

<sup>31</sup> This battery includes two questions from wave 11: the first asks if the respondent prefers to solve complex or simple problems, the second asks whether the respondent likes to have responsibility for situations that require a great deal of thinking. I rescale each from 0 to 1, then average them.

measure party competition using 2008 Gallup tracking poll data on shares of Republican and Democratic identifiers in each state (and Washington, D.C.). I use these data to compute a state-level Herfindahl Index, with higher numbers representing states with greater party competition.<sup>32</sup>

Next, turning to factors derived from motivated reasoning theory, I account for partisan directional goals with a measure of *partisan strength*, constructed by folding the survey's seven-point party identification variable into a four-point scale that ranges from independent to strong partisans.<sup>33</sup> Finally, I include a five-category measure of whether respondents *care who wins* the general election.<sup>34</sup> All else equal, those heavily invested in the election outcome should be more likely to recall considerations consistent with their preferred outcome, making them less likely to experience ambivalence (Rudolph & Popp 2007).

Rudolph & Popp (2007) argue that effortful processing should influence ambivalence differently among strong partisans than it does among weak partisans: strong partisans' confirmation and disconfirmation biases should make effortful processing result in lower ambivalence as it only serves to reinforce their biases. Conversely, weak partisans' relatively stronger accuracy goals should make effortful processing result in greater ambivalence (Millar & Tesser 1986). Rudolph & Popp's (2007) results empirically confirm these divergent expectations. Therefore, I incorporate this conditional relationship into my models with the interaction term *partisan strength*  $\times$  *political knowledge*. (Here, political knowledge is a proxy for effortful processing.)

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<sup>32</sup> The Herfindahl Index is computed by squaring each party's share of identifiers within a state, summing these squared shares, and then subtracting this sum from 1.

<sup>33</sup> Party identification is measured during wave 1 and collapsed to assign 0 for independents, 1 for partisan leaners, 2 for weak partisans, and 3 for strong partisans. Note that because I only include Democratic Party identifiers in the Clinton voter group, my partisan strength measure only contains three strength categories: leaners, weak partisans, and strong partisans.

<sup>34</sup> The wave 9 survey item reads: "How much do you care who wins the presidential election in November?" Response choices are: "A great deal," "a lot," "a moderate amount," "a little," or "not at all."



## Comparative Candidate Ambivalence in the 2008 Presidential Election

I preface my analysis with a look at comparative ambivalence data across electoral groups during the 2008 presidential election campaign. Figure 1 compares Clinton voters' ambivalence mean with those of McCain and Obama voters. The data show that, as expected, Clinton voters remain more ambivalent toward McCain and Obama than those who supported these candidates in the primaries. Perhaps indicative of the Obama campaign's relative strength, McCain voters' mean ambivalence rises slightly from June to October, while Obama voters' views toward the candidates polarize steadily; their mean ambivalence declined from 1.34 to 0.87 (a decrease of about 20% of  $AMB_{June}$ 's standard deviation). Clinton voters were significantly more ambivalent than Obama voters in September (0.53 or about 21% of  $AMB_{Sept.}$ 's standard deviation,  $t$ -test  $p < 0.05$ , two-tailed), which suggests that party loyalty pressure did not necessarily compel Clinton voters to fall in line behind Obama after he became the nominee. In October, a slightly larger gap (0.56 or 22% of  $AMB_{Oct.}$ 's standard deviation,  $t$ -test  $p < 0.05$ , two-tailed) remained between the two groups. These data suggest Clinton voters may have experienced carryover ambivalence that lasted through Election Day.

Figure 2 compares Clinton voters to independents and party identifiers who either did not vote for a major nomination candidate (Obama, Clinton, or McCain) or did not vote in their party primary. As expected, independents' mean ambivalence levels far exceed those of party identifiers throughout the campaign, whereas Clinton voters largely resemble their partisan counterparts with the exception of the post-nomination period (June to September), when mean ambivalence rises among Clinton voters but declines among other partisans and independents. In September, the difference between Clinton voters and other Democrats is 0.26 (or 10% of  $AMB_{Sept.}$ 's standard deviation,  $t$ -test  $ns$ , two-tailed), though this difference evaporates a month

later. However, the jump between June and September suggests a small but ephemeral carryover effect worthy of multivariate investigation.

### *Multivariate Analysis*

In order to better understand possible carryover ambivalence among Clinton voters, I subject the data to multivariate analysis. I model comparative candidate ambivalence both cross-sectionally and longitudinally as a function of the variables discussed above. The variables of theoretical interest in each model are the electoral group dummies, which compare each group to Clinton voters (baseline group). Group dummy coefficient estimates represent the  $AMB_{Comp}$  or  $\Delta AMB_{Comp}$  predicted mean difference between that group and Clinton voters. I also compute post-estimation predicted  $AMB_{Comp}$  or  $\Delta AMB_{Comp}$  values for each group (with all other variables held at their means) along with confidence intervals for these values.

#### Cross-Sectional Results

Table 6 reports three identically specified cross-sectional multi-level linear models (MLMs) of comparative candidate ambivalence toward McCain and Obama measured during each post-nomination survey wave (June, September, and October).<sup>35</sup> First, consider the left-hand column, which depicts the June wave model. None of the group dummy variable coefficients is significantly different from the Clinton baseline, indicating that Clinton voters' ambivalence mean is statistically indistinguishable from other groups' mean once other ambivalence predictors are held constant. Turning to the predicted ambivalence group means ( $\widehat{AMB}_{June}$ ) in Table 7, my post-estimation analysis confirms that Clinton voters

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<sup>35</sup> I use a MLM in order to adequately account for within-state clustering and nonconstant error variance across states (Steenbergen & Jones 2002).

( $\widehat{AMB}_{June} = 1.72$ ) did not experience particularly high ambivalence in June compared to other groups. These results are not surprising because, although Clinton withdrew from the nomination race on June 7, there was likely a lag between her withdrawal and the development of ambivalence among her supporters because it takes time to gather and process new information about the general election campaign. Therefore, I expect ambivalence change not to be evident immediately after Clinton withdraws.

The center column in Table 6 reports the September wave model and Table 8 reports the corresponding predicted group ambivalence means ( $\widehat{AMB}_{Sept.}$ ).<sup>36</sup> Overall, the data show mixed support for my cross-sectional ambivalence hypothesis: Clinton voters showed the highest predicted ambivalence mean of all groups ( $\widehat{AMB}_{Sept.} = 1.92$ ), though it was not significantly higher than the other groups. Consistent with expectations, the coefficients for other Democrats and Republicans are negatively signed and significant, indicating that Clinton voters were, on average, more ambivalent than partisans who did not vote for a general election candidate in the primaries. Specifically, predicted mean ambivalence for other Democrats is 1.21, which is about 0.7 ambivalence scale units lower than that of Clinton voters (about 30% of  $\widehat{AMB}_{Sept.}$ 's standard deviation). Predicted mean ambivalence for other Republicans is 1.54, or about 0.4 ambivalence scale units lower than Clinton voters' (about 15% of  $\widehat{AMB}_{Sept.}$ 's standard deviation). Though the coefficients are in the hypothesized negative direction, Clinton voters' conditional ambivalence means are not statistically different from Obama (1.64) and McCain (1.69) voters' means, which contradicts my motivated reasoning-driven expectation that these groups should be less ambivalent than Clinton voters. I refrain from interpreting the coefficient for the independents dummy variable because it was (necessarily) estimated with partisan

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<sup>36</sup> Rounding error may cause the reported model coefficients to differ slightly from the difference in predicted means between groups, but these two pieces of information are equivalent.

strength held at its mean value of 1.95, which is an infeasible value for independents who are by definition a 0 (independent) on the partisan strength scale. I do, however, estimate the predicted mean for independents (1.78), which is slightly less than Clinton voters' ambivalence mean (1.92). This is somewhat surprising given that independents typically have higher ambivalence than partisans, but alas, the difference is not statistically significant.

Turning to the October wave model in the far right column of Table 6 and the corresponding predicted group ambivalence means ( $\widehat{AMB}_{Oct.}$ ) in Table 9, Clinton voters' predicted mean ambivalence ( $\widehat{AMB}_{Oct.} = 1.69$ ) is, as expected, lower in October than it was in September. This decline suggests that Clinton voters' carryover ambivalence may have resolved itself somewhat prior to the election. Though the differences are not statistically significant, McCain voters' (1.80) and independents' predicted ambivalence means ( $\widehat{AMB}_{Oct.} = 2.00$ ) are now higher than Clinton voters' mean. All other group means remain lower than Clinton voters' mean, but the differences fail to reach statistical significance.

In sum, my cross-sectional ambivalence hypothesis is not supported: While there is limited evidence that Clinton voters are more ambivalent than some groups, particularly in September, there is no evidence that they are uniformly more ambivalent than other primary/caucus voters, partisans, or independents throughout the general election campaign.<sup>37</sup> Though interesting, cross-sectional differences between groups tell us nothing about how Clinton voters' ambivalence changes relative to other groups during the general election campaign. Therefore, I next explore the dynamics of ambivalence over the course of the campaign using longitudinal ambivalence models.

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<sup>37</sup> Small group sample sizes may be keeping some groups' standard errors higher than they would be with more observations. Tables 4a, 4b, and 4c report numbers of observations per group for each cross-sectional model.

## Longitudinal Results

Table 10 reports the results of three MLMs of comparative candidate ambivalence change -- the difference between comparative candidate ambivalence measurements across two selected waves:  $\Delta AMB_{comp} = AMB_t - AMB_{t-1}$ . Since cross-sectional comparative candidate ambivalence is measured on a scale ranging from -2 to +7, ambivalence change across waves can range from -9 to +9. The models are specified identically to the cross-sectional models reported above with the exception of an added lagged ambivalence measure ( $AMB_{t-1}$ ) that accounts for regression to the mean effects.

The left-hand column of Table 10 reports a model of ambivalence change during the nomination campaign ( $\Delta AMB_{Jan.-June} = AMB_{June} - AMB_{Jan.}$ ) and Table 11 reports the corresponding predicted ambivalence mean changes ( $\widehat{\Delta AMB_{Jan.-June}}$ ) for each group. Though this model does not directly test my longitudinal ambivalence hypothesis, I include it in order to determine whether there are group differences in ambivalence change during the nomination campaign. If such differences exist, it becomes difficult to attribute post-nomination group ambivalence change differences to nomination carryover effects. In this model, none of the group dummy coefficients is statistically different from zero, which indicates that changes in Clinton voter ambivalence were statistically indistinguishable from those of other electoral groups during the campaign's nomination phase. This null finding makes it plausible that subsequent across-wave ambivalence change differences between Clinton voters and other groups are attributable to nomination carryover effects, since Clinton's absence is the only substantive difference between the two phases. Looking at Table 11, all groups experience ambivalence decay during the nomination campaign, which seems plausible given the high amount information processing that necessarily takes place early in a campaign.

Next, I model ambivalence change between June and September – the period immediately following Clinton’s withdrawal from the Democratic nomination race. The results displayed in the center column of Table 10 and the corresponding predicted group ambivalence mean changes ( $\widehat{\Delta AMB}_{June-Sept.}$ ) depicted in Table 12 provide mixed support for my longitudinal ambivalence hypothesis: Clinton voters show the greatest predicted ambivalence change of all five groups ( $\widehat{\Delta AMB}_{June-Sept.} = +0.32$ ). The change in group ambivalence means for Obama (+0.002) and McCain (+0.24) voters are comparatively smaller, though they are not statistically different from that of Clinton voters. Of the remaining three groups, two (other Democrats and other Republicans) experience negative ambivalence change with statistically significant ambivalence mean differences from Clinton voters. Other Democrats experience an ambivalence mean decrease (-0.33) that differs from Clinton voters’ ambivalence mean increase by about two-thirds of an ambivalence unit (0.65, or about one-third standard deviation of  $\Delta AMB_{June-Sept.}$ ). Other Republicans show ambivalence change of -0.04, a 0.36 unit difference from Clinton voters. Independents’ ambivalence group mean change, though negative ( $\widehat{\Delta AMB}_{June-Sept.} = -0.07$ ), is not statistically different from Clinton voters.

Finally, I model ambivalence change for the full post-nomination period (June - October) in order to determine whether carryover ambivalence might have persisted until Election Day and potentially influenced Clinton voters’ electoral decision making. I display predicted ambivalence change group means ( $\widehat{\Delta AMB}_{June-Oct.}$ ) in Table 13. The data provide only limited support for long-lasting carryover effects: Clinton voters ( $\widehat{\Delta AMB}_{June-Oct.} = +0.11$ ), McCain voters (+0.20), and independents (+0.16) experience positive net ambivalence change during the post-nomination period. Obama voters experience the greatest decline in predicted ambivalence of all groups (-0.35), followed by other Democrats

(-0.25), and other Republicans (-0.02). Turning to the model coefficients reported in the right-hand column of Table 10, there are statistically significant ambivalence mean differences between Clinton and Obama voters (0.46) and between Clinton voters and other Democrats (0.36). Thus, during the general election campaign, Clinton voters experienced a greater ambivalence increase than three of the five other electoral groups, two of which differences with the two other Democratic groups reaching statistical significance.

To summarize, my results show only meager support for the longitudinal ambivalence hypothesis: Clinton voters' ambivalence mean increases more than any other group between June and September, but not all group mean change differences are statistically significant.<sup>38</sup> From June to October, Clinton voters exhibit significantly greater ambivalence change than two of the five electoral groups. Moreover, Clinton voters' ambivalence mean increases over the post-nomination campaign phase whereas three of the five other group means decrease. There are no discernable intergroup ambivalence change differences during the nomination phase, which suggests that the change differences observed during the general election campaign may be due to carryover effects. Clinton voters' increase in ambivalence is greater from June to September than from June to October, which indicates that they may have resolved some of it prior to Election Day.

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<sup>38</sup> Strongly Democratic Clinton primary/caucus voters may be less susceptible to experiencing ambivalence during the general election campaign than weakly Democratic Clinton voters, since stronger partisan attachments should make them more likely to "fall in line" with the party nominee. I test this by dividing Clinton voters into strong Democrats and weak Democrats/independents and re-running my models. I find no statistically significant differences between weakly Democratic/independent and strongly Democratic Clinton voters' ambivalence levels in any of my models.

## Discussion

This paper uses cross-sectional and longitudinal analysis to ascertain whether comparative candidate ambivalence is a plausible carryover effect of a competitive party nomination. I do so in the context of the 2008 presidential campaign, which featured a competitive Democratic nomination race between Barack Obama and Hillary Clinton. My results are fundamentally constrained by the small group sample sizes in the survey that severely limited the precision of my group mean estimates. The evidence, though sparse, suggests competitive nominations may increase ambivalence among supporters of nomination losers relative to some other electoral groups.

One potential explanation for my mixed results is the possibility that ambivalence peaked among thwarted voters prior to the September survey wave and I am missing more clearly defined intergroup differences due to lack of data. Therefore, future campaign panels would benefit scholars if they included more frequent surveys with larger sample sizes in order to facilitate longitudinal intergroup analyses. Despite the lack of complete and copious data, during the general election campaign, thwarted Clinton primary/caucus voters experienced greater cross-sectional ambivalence than several groups, but there were no such differences during the nomination campaign. Therefore, it is plausible that these differences are the result of carryover ambivalence from the competitive nomination, as there is no other structural change in the campaign to which these differences could be attributed. Longitudinally, Clinton voters buck the overall general election campaign trend of ambivalence decay; their mean ambivalence level increases from June to October, whereas it decreases for three of the five other groups. Though explaining why ambivalence rose so much among McCain primary/caucus voters is beyond the scope of this work, one potential explanation is his relatively weak candidacy compared to



Obama's that may have caused McCain primary/caucus voters to reassess their choice during the general election campaign.

This paper can hardly claim that the competitiveness of the 2008 Democratic nomination caused ambivalence among thwarted Clinton voters. Rather, the tenuous evidence is merely suggestive of a possible effect worthy of deeper investigation with more robust data. Clearly, competitive nominations are not the only possible cause of between-group ambivalence differences or changes in ambivalence during campaigns. Other variables may influence group differences in ambivalence or the degree to which ambivalence increases or decreases among groups during the campaign. For example, as mentioned above, candidate strength may play an important moderating role in explaining cross-sectional intergroup ambivalence differences. Moreover, contextual factors like negative racial affect toward Barack Obama may have diluted (or overstated) cross-sectional group ambivalence differences. Longitudinally, though racial bias should be a constant, party pressure to embrace Obama during the general election campaign may have triggered ambivalence among racially biased Clinton voters as this pressure conflicted with their bias. These ambivalence changes could be wrongly interpreted as competitive nomination effects when they are actually racial effects. Fortunately, the 2008-09 ANES Panel Study conducts one of the first Implicit Attitude Tests (IAT) on a representative survey sample. However, the results have not been coded, making development of a racial affect control variable prohibitively time consuming for this paper, but a task for future work.

While much prior work has focused on rather nebulous connections between competitive nominations and decreased vote share among the party nominee, this is the first paper to articulate and theoretically justify a possible within-campaign attitudinal mediator (ambivalence) between a competitive nomination race and ensuing behavioral consequences. A null result with more plentiful data might put to rest the "sour grapes" arguments scholars have

used for decades (but never directly tested) as a post-hoc theoretical explanation for negative nomination carryover effects. If competitive nominations indeed induce ambivalence among thwarted voters that lasts deep into the campaign or perhaps even through Election Day, the implications are consequential as the literature has firmly established ambivalence's meaningful impacts on campaign information processing and, ultimately, electoral behavior (see Lavine 2001 as the best overall example). Future work should investigate different ambivalence measures, particularly single-candidate ambivalence to assess whether robust and far-reaching group ambivalence differences exist during campaigns. Also, future work should investigate data from several competitive nomination campaigns across national and state offices to determine whether results are generalizable or idiosyncratic.

More broadly, ambivalence scholars should investigate why ambivalence appears to decline generally over the course of a campaign and the specific factors that contribute to its decay or persistence. We know that a great deal of learning occurs during a campaign, but we do not know the mechanism by which this influx of information reduces or increases ambivalence. Understanding this mechanism would greatly advance our understanding of the dynamics of attitudinal ambivalence during campaigns and the ways in which competitive nominations might influence them.

## Tables

Table 4. Descriptive Statistics for  $AMB_{June}$ ,  $AMB_{Sept.}$ , and  $AMB_{Oct.}$

DV	Obs	Mean	$\sigma$	Min	Max
$AMB_{June}$	970	1.62	2.41	-2	7
$AMB_{Sept.}$	1047	1.48	2.49	-2	7
$AMB_{Oct.}$	1028	1.46	2.52	-2	7

**Notes:**  $\sigma$  stands for standard deviation; **DV** stands for dependent variable; **Obs** stands for number of observations; **Min** stands for minimum; **Max** stands for maximum.

Table 5. Descriptive Statistics for  $\Delta AMB_{Jan.-June}$ ,  $\Delta AMB_{June-Sept.}$ , and  $\Delta AMB_{June-Oct.}$

DV	Obs	Mean	$\sigma$	Min	Max
$\Delta AMB_{Jan.-June}$	965	-1.1	2.39	-8	8
$\Delta AMB_{June-Sept.}$	970	-0.09	1.95	-6.5	-6
$\Delta AMB_{June-Oct.}$	954	-0.15	2.15	-8	7.5

**Notes:**  $\sigma$  stands for standard deviation; **DV** stands for dependent variable; **Obs** stands for number of observations; **Min** stands for minimum; **Max** stands for maximum.

**Table 6. Comparative Candidate Ambivalence among Electoral Groups**

	<i>AMB<sub>June</sub></i>	<i>AMB<sub>Sept.</sub></i>	<i>AMB<sub>Oct.</sub></i>
<i>Group Dummies (Clinton Voter = baseline)</i>			
Obama Voter	0.04 (0.24)	-0.28 (0.26)	-0.28 (0.24)
Other Democrat	-0.17 (0.24)	-0.71* (0.28)	-0.32 (0.24)
McCain Voter	-0.03 (0.25)	-0.11 (0.42)	0.11 (0.34)
Other Republican	-0.08 (0.22)	-0.37* (0.22)	-0.14 (0.22)
Independent**	-0.42 (0.38)	-1.18* (0.36)	-0.88* (0.35)
<i>Other Predictors</i>			
State Partisan Competition	-2.52 (3.55)	0.46 (2.53)	2.04 (2.79)
Partisan Strength	-0.36* (0.11)	-0.53* (0.11)	-0.60* (0.10)
Political Knowledge	0.09 (.07)	0.08 (0.06)	-0.07 (0.06)
Partisan Strength X Political Knowledge	-0.04 (0.03)	-0.02 (0.04)	-0.08* (0.04)
Educational Attainment	0.09 (0.09)	0.17* (0.07)	0.03 (0.09)
Need for Cognition	-0.15 (0.21)	-0.12 (0.23)	-0.30 (0.19)
Care Who Wins Election	-0.68* (0.06)	-0.75* (0.07)	-0.73* (0.07)
Constant	5.94* (2.18)	4.34* (1.52)	3.68* (1.75)
<i>Variance Components</i>			
Intercept	5.01e-10 (3.91e-9)	1.17e-7 (5.83e-7)	0.002 (0.006)
Residual	2.24 (0.04)	2.28 (0.03)	2.29 (0.04)
Number of State-Level Cases	50	50	50
Number of Individual-Level Cases	970	1047	1028

**Notes:** Table entries are multilevel estimates of cross-sectional cross-candidate ambivalence with bootstrap cluster standard errors (BCSE) in parentheses.<sup>39</sup> \**p*-value < 0.05, one-tailed. \*\*Not interpreted because this coefficient is estimated with partisan strength held at an infeasible mean value (1.95) for independents who, by definition, measure 0 (independent) on the partisan strength scale.

<sup>39</sup> Harden (2011) demonstrates that standard errors tend to be understated in clustered data models due to unmodeled within-cluster correlation, including models that employ robust cluster standard errors (RCSE). Therefore, I calculate unbiased bootstrap cluster standard errors (BCSE). In order to produce stable standard error calculations, I run 500 bootstrap replications per model (see Feng, McLerran, & Grizzle 1996).

**Table 7. Post-Estimation Predicted Group Means of  $\overline{AMB}_{June}$**

<b>Electoral Group</b>	<b><math>\overline{AMB}_{June}</math></b>	<b>95% CI</b>	<b>Obs</b>
Clinton Voters	1.72	1.36-2.07	114
Obama Voters	1.76	1.46-2.05	137
Other Democrats	1.55	1.26-1.83	174
McCain Voters	1.69	1.21-2.16	84
Other Republicans	1.64	1.36-1.92	352
Independents	2.01	1.61-2.42	109

**Notes:** Predicted means calculated using Stata's **margins** command with control variables held at their means; **s.e.** stands for standard error; **95% CI** stands for 95% confidence interval; **Obs** stands for number of observations.

**Table 8. Post-Estimation Predicted Group Means of  $\overline{AMB}_{Sept.}$**

<b>Electoral Group</b>	<b><math>\overline{AMB}_{Sept.}</math></b>	<b>95% CI</b>	<b>Obs</b>
Clinton Voters	1.92	1.51-2.32	126
Obama Voters	1.64	1.34-1.94	149
Other Democrats	1.21	0.94-1.47	185
McCain Voters	1.69	1.21-2.16	93
Other Republicans	1.54	1.32-1.76	379
Independents	1.78	1.47-2.09	115

**Notes:** Predicted means calculated using Stata's **margins** command with control variables held at their means; **s.e.** stands for standard error; **95% CI** stands for 95% confidence interval; **Obs** stands for number of observations.

**Table 9. Post-Estimation Predicted Group Means of  $\overline{AMB}_{Oct.}$**

<b>Electoral Group</b>	<b><math>\overline{AMB}_{Oct.}</math></b>	<b>95% CI</b>	<b>Obs</b>
Clinton Voters	1.69	1.34-2.05	125
Obama Voters	1.41	1.06-1.76	146
Other Democrats	1.37	1.07-1.67	182
McCain Voters	1.80	1.12-2.49	91
Other Republicans	1.55	1.29-1.82	371
Independents	2.00	1.52-2.47	113

**Notes:** Predicted means calculated using Stata's **margins** command with control variables held at their means; **s.e.** stands for standard error; **95% CI** stands for 95% confidence interval; **Obs** stands for number of observations.

**Table 10. Comparative Candidate Ambivalence Change among Electoral Groups**

	$\Delta AMB_{Jan-June}$	$\Delta AMB_{Jun-Sept.}$	$\Delta AMB_{June-Oct.}$
<i>Electoral Group Dummies</i> (Clinton Voter =baseline)			
Obama Voter	0.04 (0.23)	-0.32 (0.21)	-0.46* (0.20)
Other Democrat	-0.06 (0.20)	-0.65* (0.16)	-0.36* (0.15)
McCain Voter	0.11 (0.23)	-0.08 (0.34)	0.09 (0.27)
Other Republican	0.28 (0.20)	-0.36* (0.18)	-0.13 (0.18)
Independent**	-0.11 (0.33)	-1.00* (0.29)	-0.81* (0.32)
<i>Controls</i>			
State Partisan Competition	-0.91 (2.15)	2.02 (1.88)	2.49 (2.31)
Partisan Strength	-0.27* (0.10)	-0.31* (0.09)	-0.44* (0.09)
Political Knowledge	0.06 (0.06)	0.04 (0.04)	-0.09 (0.05)
Partisan Strength $\times$ Political Knowledge	-0.04 (0.04)	0.00 (0.04)	-0.05 (0.03)
Educational Attainment	-0.03 (0.08)	0.10* (0.05)	-0.01 (0.06)
Need for Cognition	-0.14 (0.22)	-0.08 (0.20)	-0.36* (0.17)
Care Who Wins Election	-0.50* (0.05)	-0.31* (0.06)	-0.34* (0.06)
Ambivalence <sub>t-1</sub>	-0.54* (0.03)	-0.35* (0.03)	-0.43* (0.03)
Constant	3.15* (1.32)	0.68 (1.18)	0.99 (1.42)
<i>Variance Components</i>			
Intercept	5.96e-10 (4.65e-9)	1.98e-9 (1.20e-8)	0.12 (0.48)
Residual	1.98 (0.04)	1.77 (.05)	1.87 (0.04)
Number of State-Level Cases	50	50	50
Number of Individual-Level Cases	965	970	954

**Notes:** Table entries are multilevel estimates of change in cross-candidate ambivalence with bootstrap cluster standard errors (BCSE) in parentheses (500 replications). \* $p$ -value < 0.05, one-tailed. \*\*Not interpreted because this coefficient is estimated with partisan strength held at an infeasible mean value (1.95) for independents who, by definition, measure 0 (independent) on partisan strength scale.

**Table 11. Post-Estimation Predicted Group Means of  $\Delta \widehat{AMB}_{Jan-June}$**

Electoral Group	$\Delta \widehat{AMB}_{Jan-June}$	95% CI	Obs
Clinton Voters	-1.19	-1.52-(-0.87)	114
Obama Voters	-1.15	-1.44-(-0.86)	137
Other Democrats	-1.25	-1.55-(-0.96)	174
McCain Voters	-1.09	-1.45-(-0.72)	83
Other Republicans	-0.91	-1.15-(-0.67)	350
Independents	-0.77	-1.07-(-0.46)	107

**Notes:** Predicted means calculated using Stata's **margins** command with control variables held at their means; **s.e.** stands for standard error; **95% CI** stands for 95% confidence interval; **Obs** stands for number of observations.

**Table 12. Post-Estimation Predicted Group Means of  $\Delta \widehat{AMB}_{June-Sept.}$**

Electoral Group	$\Delta \widehat{AMB}_{June-Sept.}$	95% CI	Obs
Clinton Voters	0.32	0.04-0.60	114
Obama Voters	0.002	-0.28-0.29	137
Other Democrats	-0.33	-0.53-(-0.12)	174
McCain Voters	0.24	-0.36-0.85	84
Other Republicans	-0.04	-0.21-0.13	352
Independents	-0.07	-0.35-0.21	109

**Notes:** Predicted means calculated using Stata's **margins** command with control variables held at their means; **s.e.** stands for standard error; **95% CI** stands for 95% confidence interval; **Obs** stands for number of observations.

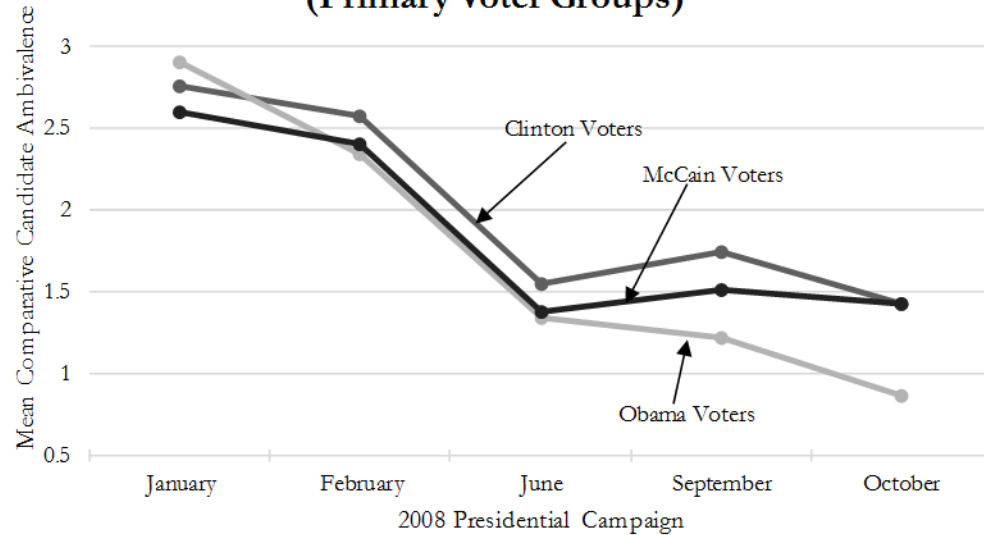
**Table 13. Post-Estimation Predicted Group Means of  $\Delta \widehat{AMB}_{June-Oct.}$**

Electoral Group	$\Delta \widehat{AMB}_{June-Oct.}$	95% CI	Obs
Clinton Voters	0.11	-0.13-0.35	114
Obama Voters	-0.35	-0.66-(-0.04)	134
Other Democrats	-0.25	-0.52-0.02	171
McCain Voters	0.20	-0.36-0.77	82
Other Republicans	-0.02	-0.25-0.21	345
Independents	0.16	-0.25-0.58	108

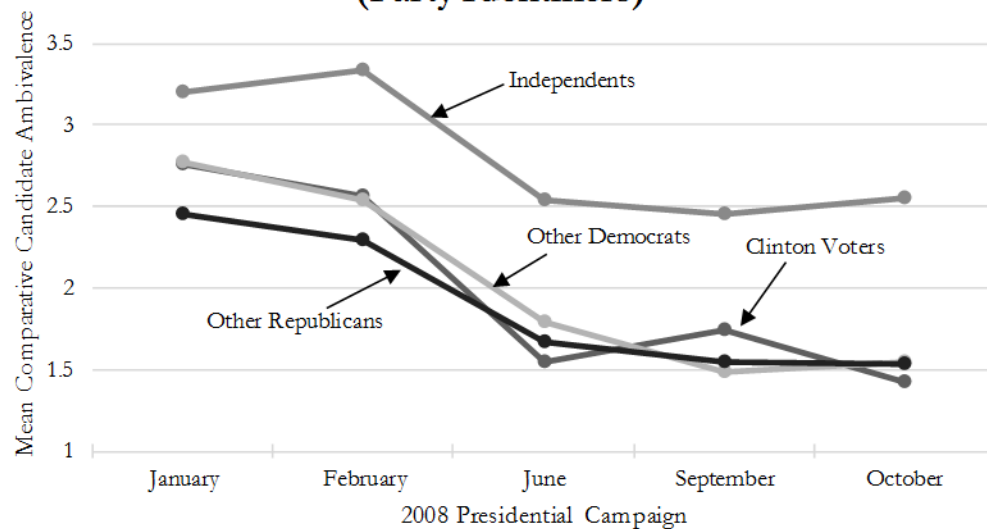
**Notes:** Predicted means calculated using Stata's **margins** command with control variables held at their means; **s.e.** stands for standard error; **95% CI** stands for 95% confidence interval; **Obs** stands for number of observations.

## Figures

**Figure 8. Mean Comparative Candidate Ambivalence over the 2008 Presidential Campaign (Primary Voter Groups)**



**Figure 9. Mean Comparative Candidate Ambivalence over the 2008 Presidential Campaign (Party Identifiers)**





### III. WHO ARE THE SCOREKEEPERS? SOPHISTICATED INDEPENDENTS AND ECONOMIC PERCEPTIONS

Democratic theory maintains that citizens should behave as rational, objective, and well-informed information processors. Individual-level behavioral studies have overwhelmingly refuted this proposition, concluding that American public opinion is alarmingly ignorant, unstable, and disorganized (Converse 1964; Delli Carpini & Keeter 1996). Converse's seminal work characterized public opinion primarily as "non-attitudes." Even partisans, who are ostensibly invested in politics to some degree, have psychological attachments to political parties which act as a "perceptual screen through which the individual tends to see what is favorable to his partisan orientation" (Campbell et al. 1960, 133). Zaller (1992) developed this idea further, suggesting that sophisticated partisans engage in "partisan resistance," in which they counterargue facts that contradict their political opinions. Similarly, political psychology's motivated reasoning theory argues that directional goals such as partisanship often bias information processing at the expense of accuracy goals (see Kunda 1990). Empirical work demonstrates the tendency for partisans to project their perceptions onto political actors (Brody & Page 1972; Martinez 1988), and make biased assessments of political figures (Bartels 2002; Goren 2002) and new information (Houston & Fazio 1989).

As scholarly attention turned to macro-level public opinion, a surprising contradiction emerged: when aggregated, public opinion appears remarkably well-informed about and responsive to economic and government performance (Kramer 1983; Page & Shapiro 1992; Durr 1993; Erikson, Stimson, & MacKuen 2002). The logic that macro-level researchers use to

bridge these seemingly discrepant results is rooted in what Stimson (2004) calls “aggregation gain” -- the tendency for aggregated data to accentuate systematic over random components. For those who do not pay attention to politics, non-attitudes are essentially random opinions. By definition, random data sum to zero when aggregated, leaving no net variance over time. Similarly, Republican and Democratic Party identifiers exhibit biased opinions in opposite directions that, when aggregated, also yield zero net variance (Bartels 2002; Erikson, Stimson & MacKuen 2002). This leaves only the well-informed, responsive, and unbiased as drivers of aggregate opinion change.<sup>40</sup>

While scholars have devoted copious theoretical attention to solving the puzzle of exactly who drives public opinion change, empirical analysis to support it is lacking. Converse (1990) suggests it is only a small group of citizens who possess the informational acuity of elites. Erikson, Stimson, & MacKuen (2002) argue that “when *Presidential Approval* changes, it is the people who are aware of the president’s activity who move.” These citizens “pay attention and respond in meaningful ways to political cues” (6-7). The authors go on to propose three models of opinion change: The “baseline” model in which change is driven by a randomly distributed subset of the electorate, the “opinion elite” model in which change is driven by a small group that pays an inordinate amount of attention to politics, and the “peripheral voter” model in which the least informed move opinion since they typically are not biased by partisan attachments. Using General Social Survey (GSS) data limited by a lack of political variables, the authors find weak support for the baseline model: “the better educated move more than others, but movement seems to come from all [demographic] strata of American society” (219).

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<sup>40</sup> This is a somewhat stylized depiction of the theory of aggregation gain. There are several caveats: Erikson, Stimson, & MacKuen (2002) warn that erroneous signals (such as responses to demagoguery) could disrupt the randomness assumption. Bartels (1996) and Duch, Palmer, & Anderson (2000) among others empirically demonstrate systematic biases in seemingly “random” aggregate data.

However, the authors readily concede that education is a crude proxy for political sophistication, so their conclusion is preliminary.

Stimson (2004) develops a more detailed theory of who moves opinion, arguing that a group of “nonideological pragmatists” called “scorekeepers” account for “virtually all [public opinion] change over time” (164). He differentiates scorekeepers from “the uninvolved,” who are uninterested in and uninformed about politics and whose opinions therefore amount to random noise, and “the passionate,” who are strongly interested in politics but hold enduring partisan commitments that make their opinions fairly constant over time. Scorekeepers care solely about outcomes such as peace and prosperity, which allows them to thermostatically change their preferences for government spending and taxes as conditions warrant (see Wlezien 1995). So, over time, scorekeepers may hold both liberal and conservative positions and support presidents from both major parties. According to Stimson, scorekeepers have two defining characteristics that make them the sole producers of systematic opinion movement: “(1) paying enough attention to respond to the common signals of politics (unlike the uninvolved); yet (2) not being so involved as to be committed always to one side (unlike the passionate)” (164).<sup>41</sup>

This paper attempts to shed some light on one of the most puzzling contradictions between macro- and micro-level public opinion studies: the seemingly simultaneous existence of individual-level noise and aggregate order. Specifically, I hope to uncover micro-level evidence of the coherent signal we see in macro-level studies. It is possible that a single, small, but as of yet undefined group of citizens make political opinion appear orderly on the macro-level, while belying underlying disorder. The central goal of this paper is to uncover whether this group of opinion-movers shares any measurable characteristics. In what follows, I make the case that Stimson’s scorekeepers are best operationalized as politically sophisticated independents. These

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<sup>41</sup> Parentheses and their contents added for clarity.

citizens should be the most likely to pay attention to politically relevant information and form objective opinions in response. Therefore, we should observe empirically that sophisticated independents' perceptions are closer than any other groups' perceptions to objective reality.

The paper is structured as follows: First, I make the case for why Stimson's scorekeepers are best operationalized as sophisticated independents. Then, I divide the citizenry into four groups that correspond to Stimson's: sophisticated independents, unsophisticated independents, in-partisans, and out-partisans. Next, I discuss theoretical expectations for each group's use of objective economic data when forming economic retrospections.

I test these theoretical expectations empirically using data from the 1980-2008 American National Election Studies (ANES) Cumulative Data File.<sup>42</sup> First, I assess baseline economic retrospection differences between the four groups mentioned above. Second, my macro-level test compares mean retrospections of each group over time. Though I do not have enough time series data points for statistical analysis, the results will visually demonstrate which group(s) best track longitudinally with the sample mean – an imperfect but necessary (for reasons explained later) approximation of true economic conditions. Third, I regress economic retrospections on a series of interactions between dummy variables for each group and the previous year's Gross Domestic Product (GDP). This will test how strongly objective economic conditions influence economic retrospections for each group. Finally, I discuss my results and their implications for future research. If my theoretical expectations hold, the implications are rather peculiar for democratic theory: those who tend to behave “correctly” (i.e., make objective judgments using full information) in American democracy are precisely those who do not take sides in the partisan structure that defines it.

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<sup>42</sup> I must exclude data from 1988 and 2002 for reasons explained in the “Data and Methods” section. There was no ANES survey administered in 2006.

## Who Are The Scorekeepers?

Building on Stimson's (2004) framework, I attempt to empirically test his theoretical inferences on both the micro and macro levels. Stimson's scorekeepers have two distinguishing characteristics: (1) they pay enough attention to politics to form accurate perceptions of the state of the political world; and (2) their perceptions are not biased by attachment to a political party. In order to test Stimson's theory, these characteristics must first be translated into measurable attributes. The latter characteristic is fairly straightforward: those without strong party attachments are typically considered independents. The former implies that scorekeepers are politically sophisticated. So, in the parlance of political science, scorekeepers are *sophisticated independents*. On the surface, this combination of characteristics seems like it would be fairly uncommon in the population since most with high political knowledge also have strong partisan attachments (Converse 1964). However, macropolitics scholars stress that opinion change that moves one party into office at the expense of the other is usually marginal rather than sweeping – on the order of a few percentage points. As a result, the size of the group that moves opinion need not be large to effect change at the margin; it could be as small as a few percent of voters (Erikson, Stimson, & MacKuen 2002; Stimson 2004).

My analysis aims to determine whether scorekeepers' opinions track with objective reality better than those of Stimson's two other groups: the "uninvolved" and the "passionate." Following Stimson's logic, the uninvolved are best described as unsophisticated independents because they lack partisan ties (independent) and political information (unsophisticated). Partisans embody Stimson's characterization of the passionate.

In order to test my theory, I must find a perception that also has an objective reference from which to measure "error." Unfortunately, most attitudes, perceptions, and opinions captured in surveys (e.g., preferred level of government spending) lack an objective reference

point. However, economic retrospections -- a fundamental influence on electoral decision making -- *do* have a clear reference point from which to measure bias that the mass media consistently relay to the public: GDP (Kramer 1983; Nadeau et al. 1999).<sup>43</sup> If sophisticated independents are behaving as scorekeepers, GDP should inform their economic retrospections more than those of other groups. As Stimson (2004) puts it, “the net perception of the economy, neither biased nor ignorant, is driven wholly by the scorekeepers” (165).

### **Expectations for Four Groups**

For purposes of this analysis, I divide the citizenry into four groups: sophisticated independents, unsophisticated independents, out-partisans, and in-partisans. I should observe clear intergroup differences across three main areas of investigation: (1) groups’ baseline economic retrospections; (2) how each group’s retrospections track with mean retrospections over time; and, most importantly, (3) the degree to which objective economic conditions inform each group’s retrospections.

#### *Partisans*

Campbell et al. (1960) first proposed that partisans see politics through a “perceptual screen” that distorts reality in favor of what is consistent with their party orientation. Subsequent work suggests that partisans tend to engage in two processes that cause them to bias their perceptions: First, they tend to selectively expose themselves to and accept information that reinforces their partisan biases (Sears & Freedman 1967; Zaller 1992). Second, motivated reasoning theory states that those with directional goals (e.g., partisanship) tend to bias their

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<sup>43</sup> I chose retrospections over prospectors because retrospections are measured when the objective baseline (GDP) is already widely known; it is a perception of past performance that is either correct or incorrect at the time of the survey instead of a prediction of future performance that has no objective comparison at the time of the survey.

opinions in favor of the goal (and at the expense of accuracy) by either ignoring contradictory information altogether or attempting to discount it (e.g., Kunda 1990; Lodge & Taber 2000). Therefore, I develop the following expectations for partisans: (1) Partisans should exhibit *biased* retrospections compared to independents. Since their party is in power, in-partisans should be much more likely than out-partisans and somewhat more likely than sophisticated and unsophisticated independents to hold positive retrospections, all else equal. Conversely, since their party is not in power, out-partisans should be much more likely than in-partisans and somewhat more likely than sophisticated and unsophisticated independents to hold negative retrospections, all else equal;<sup>44</sup> (2) on the macro level, in-partisans' mean retrospections should be higher than the sample mean over time whereas out-partisans' retrospections should be lower than the sample mean; (3) in-partisans and out-partisans' retrospections should be less *responsive* to changes in GDP than sophisticated independents' retrospections. In other words, partisans' retrospections should change at a slower rate as a function of GDP than sophisticated independents' retrospections do. Additionally, partisans' retrospections should exhibit signs of *bias* in the form of shifted predicted probability graphs relative to sophisticated independents.

#### *Unsophisticated Independents*

Unsophisticated independents face a different situation than partisans; they are not biased, but they are also not informed. This leads me to develop the following expectations for this group: (1) since they lack directional goals, I do not expect unsophisticated independents' retrospections to be biased. On average, I expect their retrospections to be similar in relation to

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<sup>44</sup> Empirical evidence to date shows that partisans have distorted economic perceptions. For example, Bartels' (2002) analysis of the 1988 post-Reagan ANES panel study finds that strong Democrats perceived unemployment to be higher than it really was whereas Republicans correctly perceived a moderate decline in unemployment during the Reagan Administration. More dramatically, inflation declined from 13.5 to 4.1 percent but 50 percent of strong Democratic respondents felt that inflation had actually increased and only eight percent of strong Democrats felt that inflation had declined.

partisans as those of sophisticated independents (see below). While I expect their individual-level data to be less objectively accurate, I do not expect to find evidence of systematic bias in the group retrospection means;<sup>45</sup> (2) on the macro level, I expect unsophisticated independents to have retrospections that track fairly closely with sample mean retrospections. Though their individual-level retrospections should be more randomly distributed across the survey responses or perhaps modally concentrated at a response such as “about the same,” once aggregated, the randomness should cancel, leaving behind a fairly accurate signal from the small subset of unsophisticated independents who somehow manage to pick up on the overall state of the economy; (3) though ostensibly motivated by accuracy goals, unsophisticated independents lack sufficient information to form objective perceptions, at least most of the time. Thus, GDP should poorly predict unsophisticated independents’ retrospections relative to sophisticated independents. In other words, like partisans, unsophisticated independents should be less responsive to changes in GDP than their sophisticated counterparts.

### *Sophisticated Independents*

Sophisticated independents lack partisan bias and therefore should be motivated solely by accuracy goals. Unlike unsophisticated independents, they possess enough political information to reliably achieve them. As a result, I generate the following expectations for sophisticated independents: (1) sophisticated independents should hold unbiased retrospections -- that is, they should be more likely than out-partisans and less likely than in-partisans to hold positive retrospections, all else equal. Symmetrically, sophisticated independents should be more

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<sup>45</sup> It is highly unlikely that *all* unsophisticated independents are uninformed about the state of the economy. The theory of aggregation gain tells us that it only requires a handful of unsophisticated independents who stay informed about the economy to make the group mean appear to be in-step with economic conditions. Uninformed opinions are random and therefore cancel when aggregated, leaving a “signal” driven by the few who pay attention to economic conditions.



likely than in-partisans and less likely than out-partisans to hold negative retrospections, all else equal; (2) since I anticipate that sophisticated independents hold unbiased retrospections, on the macro level, their retrospections mean should track more closely with the sample mean than partisans' retrospections; it should be consistently lower than in-partisans' mean and higher than out-partisans' mean across time; (3) economic retrospections should be more responsive to GDP for sophisticated independents than for any other group.

### **A Note about Expectations**

It is important that I do not overstate my theoretical expectations. For example, it would be unreasonable to expect that *only* sophisticated independents use GDP to form retrospections. Kunda (1990) cautions that motivated reasoning bias is limited by one's ability to reasonably justify it. While the partisan motive is strong, it is not absolute; accuracy goals still exist to some degree for even the strongest partisans to minimize the cognitive dissonance from holding opinions inconsistent with reality (see Festinger 1957). So, when the economy moves overwhelmingly in one direction, partisans should find it increasingly difficult to maintain their biased opinions and adjust their perceptions to be somewhat more accurate in response. Also, despite any partisan's best efforts to only consume information that suits them, it is difficult to avoid objective economic data altogether (Zaller 1992). Empirically consistent with Kunda's (1990) and Zaller's (1992) theories, Gerber & Green (1999) and Page & Shapiro (1992) demonstrate that citizens exhibit parallel opinion change across partisan and demographic groups. That is to say, all groups respond to *some* degree to economic data but the magnitude of that response may vary across groups. Therefore, we should expect some Bayesian updating among even the most die-hard partisans, who should have some desire to maintain a roughly accurate perception of their party's stewardship of the economy to avoid sounding delusional

(Achen 1989; 1992; Gerber & Green 1999), especially given that objective economic conditions are widely disseminated by the media (Nadeau et al. 1999). As a result, my theory prescribes no absolutes, only differences in degree -- we should see the *most* accurate and responsive perceptions among sophisticated independents.

Likewise, it is not reasonable to assume that the entire group of unsophisticated independents is unaware of national economic health. In other words, my sophistication measure is not going to perfectly segregate those who perceive the state of the objective economy from those who do not. As we know from macro-level opinion change theory, only a small responsive subset is necessary to make a group signal appear orderly. Therefore, I do not expect to see noisy *aggregate* data from unsophisticated independents. Instead, the primary distinction between my theoretical expectations for unsophisticated and sophisticated independents' retrospections is that the former should be demonstrably less responsive to GDP changes than the latter.

## Data and Methods

My analysis uses pooled data from the ANES Cumulative Data File for federal election years from 1980 to 2008.<sup>46</sup> My dependent variable for my micro-level analyses is the five-point ordinal economic retrospections scale, which asks respondents to rate the performance of the economy over the past year as either "much worse," "somewhat worse," "about the same," "somewhat better," or "much better" than the previous year.<sup>47</sup> Since this is an ordinal scale, both

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<sup>46</sup> I omit 1988 data because there were no post-interview information ratings in that year's survey. Year 2002 data are omitted due to insufficient income data. There was no ANES survey administered in 2006.

<sup>47</sup> The question reads: "Now thinking about the economy in the country as a whole, would you say that as compared to a year ago, the nation's economy is better, about the same, or worse?" Respondents who choose either "better" or "worse" are asked to further specify the degree.

of my micro-level analyses use ordered probit models estimated with maximum likelihood estimation.

I construct my four categories of respondents as follows: The ANES seven-point party identification scale asks respondents to classify themselves as either strong or weak partisans or as an independent. Interviewers then ask respondents who identify themselves as independents to state whether they feel closer to one of the major parties and if so, which party. This results in three types of independents: Republican-leaning, Democrat-leaning, and pure independent. I transform this party identification scale into a measure of partisan strength by folding “strong” Republicans and Democrats into one category, “weak” partisans and partisan-leaning independents into another, and pure independents into a third. I classify partisan-leaning independents as partisans because of research suggesting leaners’ behavior is statistically indistinguishable from weak partisans (Keith et al. 1992). Since independent leaners can be expected to exhibit partisan bias, it would be unwise to consider them candidates to be scorekeepers.<sup>48</sup>

After separating the sample by strength of partisanship, I next control for presidential in-party status among partisans, creating categories for in-partisans and out-partisans. Consistent with motivated reasoning theory (see Kunda 1990) and selective information exposure (Zaller 1992), I anticipate significant in-party/out-party effects on economic retrospections (Kinder and Mebane 1983). Specifically, in-partisans’ economic assessments should be biased in favor of the incumbent and out-partisans’ economic assessments should be biased in favor of the out-party.

Finally, I divide independents by sophistication level. Scholars have heavily debated sophistication both conceptually and operationally. Conceptually, the consensus is that sophistication is the degree of cognitive complexity in one’s thinking about politics.

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<sup>48</sup> Though, by necessity, I must regrettably renege on this decision for my macro-level analysis.

Operationally, this implies a high level of political knowledge (both factual and conceptual), awareness, and interest (Zaller 1992). For my purposes, political knowledge appears to be most germane, since scorekeepers have to be reasonably informed on the state of the economy to produce accurate perceptions. Though economic growth is not political information per se, it is, as Bartels (2002) calls it, “politically relevant” since it informs citizens’ political attitudes and behavior. Often, researchers build composite measures using several political information recall questions (e.g. “Who is the Chief Justice of the United States?”). However, the ANES Cumulative Data File does not contain many recall questions and the ones it does contain are limited to only a handful of survey years. Therefore, like much prior research, I choose to measure sophistication using the interviewer assessment of the respondent’s level of political information.<sup>49</sup>

I chose the interviewer assessment for two reasons: First, Zaller (1985) concludes that it is a “highly effective” sophistication measure due to its high reliability and low bias compared to more complicated information scales (17-18). This is not surprising since the interviewer ostensibly takes into account *all* information from the interview in formulating a placement, including information not captured in the survey data. Second, the descriptive data do not support Gomez and Wilson’s (2001) claim that the interviewer assessment suffers from “the fatal defect of insufficient variance” (904, footnote 4). On the contrary, the dispersion of the data ( $\sigma = 1.05$ ) indicates that they contain meaningful variation. Gomez and Wilson’s (2001)

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<sup>49</sup> Since my sophistication measure is a subjective interviewer assessment, I test for biased assessments of two groups that have historically faced discrimination – blacks and women. First, I compare the sophistication mean for black respondents when the interviewer is white with the mean when the interviewer is black. Second, I compare the sophistication mean for female respondents when the interviewer is male with the mean when the interviewer is female. The model also controls for respondent-reported education level. The sophistication mean for white interviewers and black respondents is lower ( $-0.35, p < 0.05$ ) than the mean for black interviewers and black respondents. The sophistication mean for male interviewers and female respondents is *higher* ( $0.20, p < 0.05$ ) than the mean for female interviewers and female respondents. It is impossible to know if true bias exists or what direction it takes since there is no objective baseline with which to compare the assessments. For example, it could be the case that black interviewers are biasing their assessments of black respondents positively. However, these results suggest further investigation is in order.

claim that there is a tendency to call “all but the most exceptional respondents ‘average’” (904, footnote 4) does not square with the distribution of the actual data (see Appendix B, Figure A) in which 11% of respondents are coded as having very high information, 6.6% as very low, 19.7% as low, 25.2% as high, and 37.6% as average.

The interviewer assessment classifies respondents’ level of political information as “very high,” “high,” “average,” “low,” or “very low.” I label respondents in the “very high” and “high” categories as “sophisticated” and those “average” and below as “unsophisticated.” I label the two highest categories as “sophisticated” because: (1) Zaller (1985) finds that interviewers have the weakest discriminatory power between these two categories, so they likely overlap somewhat; and (2) Stimson (2004) posits that scorekeepers need only be “reasonably well informed.” Thus, a sophisticated independent would seem to require merely a “high” level of political information and not a “very high” level. Controlling for sophistication gives me four groups of respondents included in my model as dummy variables: sophisticated independents (baseline category; 2.4% of sample), unsophisticated independents (9.1% of sample), in-partisans (41.6% of sample), and out-partisans (46.9% of sample).

My analysis consists of three parts. First, I use an ordered probit model to test whether there are statistically significant group differences in economic retrospections and whether these differences are in the theoretically expected direction. Comparatively speaking, in-partisans should have the most positive retrospections of all groups, out-partisans should have the most negative retrospections and independents should be between the two, all else equal. I account for year clustering in the data by (1) allowing the year intercepts to vary (year dummies); and (2) cluster-correcting the standard errors for the year clusters. Following Conover, Feldman & Knight (1986), I include a series of demographic predictors of economic retrospections: race,

gender, education level, family income, age, and region.<sup>50</sup> I also include employment status since unemployed citizens should be more likely to evaluate the economy negatively, all else equal. To ensure that my sample represents the population as accurately as possible, I weight the data using ANES-provided post-stratification weights.

Next, I complement my micro-level group analysis with a macro-level group analysis that charts how closely each group's mean economic retrospections track with the overall sample mean over time.<sup>51</sup> For each survey year, I take each group's mean economic retrospection value and graph it along with that year's overall survey mean. Unlike the micro-level analysis described above, this one gives the reader a visual comparison of retrospection group means and how they change over time. While I do not have enough aggregate data to perform a time series regression, I can visually and numerically assess the degree to which each group's perceptions deviate from the sample mean and determine whether there are statistically significant differences between group means at each survey year data point. To recapitulate my theoretical expectations, I should see independents (both sophisticated and unsophisticated) follow the sample mean retrospection most closely. In-partisans should be positively biased and out-partisans should be negatively biased relative to the sample mean.

There are two caveats to my macro analysis. The first is that the sample mean likely does not capture truly "objective" economic retrospections. As Bartels (1996) and Duch, Palmer, & Anderson (2000) show, there are often systematic biases in data that persist when it is

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<sup>50</sup> There are no explicit expectations behind these demographic controls; I am including them because they were included as controls in Conover, Feldman, & Knight (1986), which attempted to determine factors that shape economic retrospections.

<sup>51</sup> Mapping GDP data onto the retrospections scale using predicted values after regressing retrospections on GDP appears at first glance to be a good way to uncover "objective" retrospections to facilitate group comparisons. However, GDP does not accurately transfer to the retrospections scale due to biases in the distribution of retrospection responses: retrospections are consistently negatively biased such that the maximum sample mean (in a year with 3.7 percent GDP growth) is 3.26, which is between the "stayed the same" and "somewhat better" responses. Therefore, I use the sample mean retrospection as a representation of the aggregate "signal" on economic retrospections.

aggregated. For example, Democrats may outnumber Republicans in the sample (and by extension, the population) such that their perceptual bias disproportionately influences the aggregate retrospection mean relative to Republicans – in other words, partisan bias may not sum to zero when opinion data are aggregated across all respondents. Though the sample mean may be biased, it does capture the aggregate retrospection “signal,” making it a suitable, if imperfect, baseline for comparing group retrospection means.

The second is that the sophisticated independents sample size is exceedingly small in most years -- averaging 38 respondents per survey year, but some have samples under 30.<sup>52</sup> This gives me unacceptably low confidence that the sophisticated independents sample mean accurately captures the group population mean and makes the population mean estimate susceptible to outlier effects. Since my longitudinal group comparisons rely on the sample mean accurately reflecting the population mean for each group, I must broaden my definition of independents to include partisan leaning-independents for purposes of this analysis. These respondents answer the initial party identification question with “independent,” but identify a major party to which they identify with more closely than the other. Though less than ideal (given previously mentioned research demonstrating that leaners behave much like partisans), this change will give me a large enough sample to guarantee accurate group means without compromising the core definitions of the groups.

My third analysis tests for group differences in how objective economic information conditions retrospections. To capture objective economic performance over the past year, I use annual percent change in GDP data from the U.S. Department of Commerce’s Bureau of Economic Analysis because it is widely accepted as the strongest measure of the United States’

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<sup>52</sup> These numbers represent about 2-3% of each survey’s sample. Assuming unbiased sampling practices, this means the percentage of sophisticated independent in the U.S. population is about 2-3%, which is consistent with Stimson’s theory that only a small percentage of the overall population effects change at the margins.

overall economic health. The annual percent change in GDP (or GDP growth rate) is computed by dividing the current year's inflation-adjusted GDP (in dollars) by the previous year's GDP. Since the ANES surveys that compose the Cumulative Data File are typically administered during the fourth quarter of the survey year, I use the GDP annual percent change value for each survey year to predict retrospections of that year's economy.<sup>53</sup> I expect the past year's GDP growth rate (hereafter, GDP) to predict sophisticated independents' retrospections better than all other groups' retrospections.<sup>54</sup>

My primary variables of interest are interactions between each group dummy variable and GDP. These interactions will reveal the degree to which yearly GDP conditions economic retrospections for each group. I display my model results in a table accompanied by a series of graphs to visually depict how each group's predicted probability of choosing three retrospection responses changes across GDP levels. If the data support my theoretical expectations, I should observe more steeply sloped predicted probability lines across GDP values for sophisticated independents than for other groups. Steep lines indicate sharper responsiveness to objective conditions than flatter ones. More precisely, steep lines indicate that predicted probabilities of a given retrospection change more quickly as a function of GDP than flatter lines. Since GDP growth is a yearly measure that by definition only varies by survey year, it will perfectly correlate with year dummies if these are included in this model, making it unidentified. As a result, I choose to include GDP and drop the year dummies since GDP is my primary year-to-year variable of interest. This also allows me to construct predicted probabilities for retrospections

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<sup>53</sup> Since the survey is usually administered during Q4 of the survey year, one might suspect that annualized data from Q4 of the year prior to the survey year through Q3 of the survey year would correlate better with retrospections than survey year GDP. After testing this, I found that it did not; yearly GDP growth correlated best with retrospections.

<sup>54</sup> The annual GDP growth rate is computed by dividing the current year's GDP (in dollars) by the previous year's GDP.



across GDP levels. Like my other micro-level analysis, I use standard errors cluster-corrected for the year clusters, ANES post-stratification weighting, and the same set of demographic controls.

### **Micro-Level Group Economic Retrospection Differences**

First, I assess whether there are discernable group differences in economic retrospections. The results displayed in the left-hand column of Table 14 strongly support my theoretical expectations: Presidential in-partisans show a statistically significant mean economic retrospection difference (0.29) with sophisticated independents. Also, as expected, out-partisans' retrospection mean is significantly lower (-0.24) than that of sophisticated independents. In sum, in-partisans view the economy more positively and out-partisans more negatively, on average, than sophisticated independents.<sup>55</sup> As expected, unsophisticated independents and sophisticated independents do not have statistically significant differences in their retrospections.

To more completely interpret these nonlinear results, I compute predicted probabilities across retrospection categories for each group, holding all other group dummies at 0 and all other predictors at their means.<sup>56</sup> I expect out-partisans to have significantly higher predicted probabilities of negative retrospections (i.e., “much worse,” “worse”) than in-partisans and sophisticated/unsophisticated independents, though I expect the differences to be greatest between out- and in-partisans. Likewise, I expect in-partisans to have significantly higher predicted probabilities of positive retrospections (i.e., “much better,” “somewhat better,” and

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<sup>55</sup> This effect increases with stronger party ties. In a model that separates partisans by strength of partisanship, the coefficient is greater for strong in-partisans (.51) than other in-partisans (.23). Similarly, the coefficient for strong out-partisans is of greater absolute value (-.35) than for other out-partisans (-.14).

<sup>56</sup> Predicted probabilities computed using the **prvalue** command from the **SPost** add-on package for Stata (see Long & Freese 2005).

“about the same”)<sup>57</sup> than out-partisans and sophisticated/unsophisticated independents, with the largest differences again between out- and in-partisans. I do not expect to find any systematic difference between sophisticated and unsophisticated independents’ retrospections since unsophisticated independents should have no directional bias to pull their retrospection probabilities away from their sophisticated counterparts. Table 15 reports the predicted probabilities for each group and Table 16 reports between-group predicted probability differences. As expected, out-partisans and in-partisans demonstrate statistically significant differences across all but one retrospection category and all differences are in the expected direction relative to each positive/negative category. For example, in-partisans have a 0.13 lower predicted probability than out-partisans of choosing the “much worse” retrospection (0.11 vs. 0.24,  $p < 0.05$ ) and a 0.10 higher predicted probability of choosing the “somewhat better” response (0.20 vs. 0.10,  $p < 0.05$ ).

Sophisticated independents’ predicted probabilities differ from out-partisans’ in the predicted directions across all retrospection categories, with these differences reaching statistical significance in two of the five categories: the probability of an “about the same” retrospection is 0.04 lower among out-partisans (0.32 vs. 0.36,  $p < 0.05$ ) and the probability of a “somewhat better” retrospection is 0.04 lower (0.10 vs. 0.14,  $p < 0.05$ ). Similarly, sophisticated independents’ predicted probabilities differ from in-partisans’ in the predicted direction across all retrospection categories as well, with two of the five differences reaching statistical significance. In-partisans’ predicted probability of a “much worse” retrospection is 0.06 lower than sophisticated independents’ (0.11 vs. 0.18,  $p < 0.05$ ). Consistent with directional predictions, in-partisans also

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<sup>57</sup> The sampling distribution of economic retrospections indicates that citizens tend to be pessimistic about economic performance, all else equal. Though “about the same” would seem to indicate a neutral response, the descriptive data indicate that the distribution of responses is negatively skewed (see Appendix B, Figure B); the “much better” response is a seldom chosen fringe category (4.3 percent of the distribution). Thus, the “about the same” response tends to reflect a slightly positive retrospection.

have a 0.06 higher probability than sophisticated independents of selecting “somewhat better” (0.20 vs. 0.14,  $p < 0.05$ ). However, in-partisans are careful not to distort reality too much, with only a 0.02 greater predicted probability of choosing “much better” (0.03 vs. 0.01, *ns*) than sophisticated independents.

Also as expected, sophisticated and unsophisticated independents show nearly identical predicted probabilities for each retrospection category. The magnitude and direction of predicted probability differences between sophisticated independents and partisans support the contention that independents possess objective retrospections – or at least retrospections that are in the middle of the gap between in- and out-partisans. This preliminary analysis gives me a good indication that groups may formulate their retrospections differently – notably, that partisans possess biased retrospections relative to independents.<sup>58</sup> Next, I investigate group retrospections from a macro perspective.

### **Macro-Level Analysis of Economic Retrospection Group Means**

Though the preceding analysis suggests important differences between the retrospections of sophisticated independents, unsophisticated independents, and partisans, it does not give us a visual perspective on these differences nor does it describe differences in the groups’ mean retrospections over time. I display the time series for each group’s retrospection means in several graphs (Figures 10-13) with solid lines representing the sample means and broken lines around

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<sup>58</sup> While differences between sophisticated independents and other groups are, for the most part, not statistically significant, it would probably be unrealistic to expect these group differences to be significant in *every* retrospection category since the response to the retrospection question is zero-sum – group differences reflect merely a tendency for one group to shift responses toward one end of the scale or the other relative to another group. In other words, some in-partisans who would otherwise choose “somewhat worse” choose “about the same” due to their partisan bias, but this may not be enough to create a statistically significant difference in the predicted probability of this category with another group, particularly if would-be “much worse” retrospections also shift to “somewhat worse” to replace those that shifted to “about the same.”

them denoting their 95% confidence interval. Lines whose boundaries do not overlap signify significant differences between group and sample means.

Recall that the fundamental expectation of Stimson's theory is that partisans should exhibit biased retrospections across time. Figures 10 and 11 show that this expectation is supported by the macro-level data: the means for in-partisans remain higher than the sample mean throughout the series and the out-partisan means remain lower. Thus, in-partisans are consistently more positive about the economy than the sample mean and out-partisans consistently more negative. The confidence interval lines show that, for the most part, these differences are statistically significant. Stimson's theory posits that these partisan biases should roughly cancel one another. In order to test the theory, I look at each group's absolute deviation from the sample mean in each survey year and sum these across the time series to form a "total deviation score." The higher this score, the more the group deviated from the sample mean retrospection. To prevent a large deviation or two from skewing my results, I use the mean absolute deviation from the sample mean to compare across groups.

The data depicted in Table 17 show that in-partisans exhibit greater cumulative bias (3.72) from the sample mean than out-partisans (2.90). Additionally, in-partisans show slightly more bias on average (.29) than out-partisans (.22). While in-partisans and out-partisans exhibit consistently biased retrospections over time, the biases do not cancel one another when aggregated. Instead, consistent with my micro-level results, there is a net positive longitudinal in-partisan bias. Rather than indicating greater in-partisan than out-partisan bias, this disparity may be due to the fact that out-partisans disproportionately influence the sample mean since they make up a greater share (47%) relative to in-partisans (42%) of the total sample used to compute it. However, assuming my sample represents the U.S. population, the sample mean is supposed

to indicate the true retrospection “signal.” Therefore, Stimson’s theory of symmetrical partisan bias is not fully supported by the data – it appears some bias persists in the aggregate data.

According to my theory, both sophisticated and unsophisticated independents’ retrospection means should mirror the sample mean better than the other groups. Unsophisticated independents’ retrospections should be more random on the individual-level than sophisticated independents’ retrospections (perhaps more modal or more dispersed, depending on the survey question), but not systematically biased either positively or negatively. As expected, Figure 12 shows that sophisticated independents’ retrospection means track the sample mean fairly well as the two lines overlap considerably. Statistically significant differences between the two series only occur in three of the 13 survey years: 1984, 1990, and 1994. Likewise, Figure 13 shows that unsophisticated independents’ retrospection means also appear to match the sample mean retrospection much more closely than their partisan counterparts and slightly more closely than sophisticated independents. Unsophisticated independents’ retrospection means only show statistically significant differences from the sample mean in 1984 (barely) and 1994. Table 17 shows that sophisticated independents actually have a slightly higher total absolute deviation score (1.50) than unsophisticated independents (1.33) and a slightly higher mean absolute deviation score (0.12) than their unsophisticated counterparts (0.10). With this perspective in mind, I now turn to whether partisan bias or a lack of information limits the degree to which economic growth rates condition retrospections.

### **Intergroup Differences in How GDP Conditions Economic Retrospections**

In order to assess whether sophisticated independents evaluate the economy more objectively than other groups, I introduce a second model featuring interactions between prior-year GDP and each group dummy variable (again using sophisticated independents as the

baseline group). My results are displayed in the right-hand column of Table 14. The reported interaction coefficients cannot be directly interpreted in non-linear models such as this because they are conditional on the values of the other independent variables (see Ai & Norton 2003; Ai, Wang, & Norton 2006). Therefore, to correctly interpret them, I graph the predicted probabilities of three retrospection response categories (“somewhat better,” “somewhat worse,” and “about the same”) across a range of feasible GDP values. These graphs provide a visual depiction of how each group’s retrospection mean responds to changes in GDP. In each graph, the y-axis depicts the predicted probability of the response category and the x-axis depicts GDP values from -4% growth to 8% growth, plotted at half-percent intervals. To help visualize statistically significant group differences, 95% confidence interval boundaries are denoted by two broken lines around each predicted probability line. Lines whose boundaries do not overlap signify significant between-group differences in predicted probability at a given GDP level.

These interaction graphs help me test two major theoretical expectations: (1) that sophisticated independents’ retrospections should be more *responsive* to GDP growth levels than all other groups. This means that the predicted probability lines for sophisticated independents should be more steeply sloped than those of all other groups across all levels of GDP; and (2) that partisans’ retrospections should show signs of *bias* across GDP growth levels in the expected direction. In terms of the graphs, this means that in-partisans’ predicted probability peaks for each retrospection should occur at lower GDP levels than they do for sophisticated independents and out-partisans’ probability peaks should occur at higher GDP levels than they do for sophisticated independents. This essentially amounts to a leftward (in-partisans) or rightward shift (out-partisans) in the predicted probability distribution relative to sophisticated independents. Figure C in Appendix B visually illustrates both responsiveness and bias.

Figures 14, 15, and 16 compare predicted probabilities for sophisticated independents' retrospections with those of in-partisans, conditioned on GDP growth. Following the lines for each group from left to right in Figure 14, we see a markedly steeper predicted probability line slope for sophisticated independents -- clear evidence of sharper responsiveness to GDP as it increases along the x-axis. The same general pattern is evident in Figures 15 and 16 -- the rise in predicted probability of a "somewhat better" (Figure 15) and "about the same" (Figure 16) response is more gradual for in-partisans (moving left to right in the figure) than it is for sophisticated independents, which indicates that their retrospections respond more sharply to change in GDP.

The predicted probability graphs highlight several notable statistically significant intergroup differences: Figure 15 shows that in-partisans are significantly more likely than sophisticated independents to choose the "somewhat better" retrospection when GDP growth is below 3.0% (generally considered below average growth), indicating the in-partisan tendency to see the economy as rosier than it actually is. Similarly, Figure 16 shows that in-partisans are much more likely than sophisticated independents to say the economy is "about the same" when it is actually contracting ( $GDP < 0$ ).

In terms of bias, in-partisans' predicted probabilities peak at lower GDP growth levels than sophisticated independents' probabilities in two of the three analyzed retrospection categories ("somewhat worse" and "about the same"). For example, in Figure 14, the predicted probability of a "somewhat worse" retrospection peaks at a GDP growth rate between -0.5% and 0%. For sophisticated independents, this probability peaks when GDP growth is about 2%, indicating that in-partisans have a distorted perception of what constitutes "somewhat worse" economic performance. In sum, the "partisan screen" imparts an unresponsive and skewed interpretation of economic conditions for in-partisans relative to sophisticated independents.

Next, Figures 17, 18, and 19 compare sophisticated independents' retrospections with those of out-partisans. Out-partisans present a similar comparative picture to in-partisans: their predicted probability lines for all three retrospection categories are noticeably less steep than those of sophisticated independents, indicating less responsiveness to GDP. In Figure 17, the predicted probability line for sophisticated independents resembles a parabola with a fairly well-defined peak where  $GDP = 2\%$ , whereas the line for out-partisans has a markedly smaller slope. This poor responsiveness to GDP among out-partisans results in a particularly small range of predicted probabilities for the "somewhat worse" retrospection category -- about 0.15-0.32, as opposed to about 0.01-0.32 for independents. Figures 18 and 19 compare the same groups for the "somewhat better" and "about the same" retrospection categories respectively. This time, the predicted probability line slopes indicate that out-partisans are more responsive than they were in Figure 17, but still less responsive than sophisticated independents.

Like in-partisans discussed above, out-partisans' flatter probability lines result in statistically significant predicted probability differences with sophisticated independents: For example, the statistically significant separation between the groups' predicted probability lines in Figure 18 indicates that out-partisans are significantly more reluctant than sophisticated independents to call an economy with greater than 3% economic growth "somewhat better." Similarly, Figure 17 shows that out-partisans are significantly more likely than sophisticated independents to report the economy as "somewhat worse" when growth rates are at 5% or greater.

In terms of bias, out-partisans' predicted probabilities peak at higher GDP levels than sophisticated independents' probabilities for two of the three analyzed retrospection categories ("somewhat better" and "about the same").<sup>59</sup> For example, in Figure 19, the predicted

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<sup>59</sup> Since some of the peaks occur outside of the feasible GDP range, they may not be visible in the figures presented.



probability of an “about the same” response peaks at about 6.5% GDP growth whereas for sophisticated independents, the peak is around 4% GDP growth. In contrast to sophisticated independents, out-partisans exhibit biased retrospections that are less responsive to objective economic conditions.

My theoretical expectations specify that the line depicting predicted probabilities across levels of GDP should be significantly steeper for sophisticated independents than for unsophisticated independents, indicating higher responsiveness to GDP in forming retrospections. The data presented in Figures 20, 21, and 22 support my expectations: sophisticated independents’ predicted probability line is significantly steeper than unsophisticated independents’ line across GDP values for all three retrospection categories. For example, in Figure 22, the predicted probability line for sophisticated independents rise and falls sharply in comparison to the line for unsophisticated independents, which shows a much more gradual rise across GDP levels. Thus, unsophisticated independents are less responsive to GDP and hence incorporate it less than sophisticated independents do when forming their retrospections. Rather than being attributable to a “partisan screen,” differences in responsiveness between these groups are solely a function of higher sophistication – the only substantive difference between them.

The sharper responsiveness of sophisticated independents relative to unsophisticated independents creates statistically significant differences between the two groups’ retrospection probabilities at various GDP levels. For example, in Figure 21, once GDP reaches about 4% annual growth (generally considered fairly strong growth), sophisticated independents become significantly more likely than unsophisticated independents to give the “somewhat better” retrospection, suggesting they are more in tune with actual economic conditions. Conversely,

when GDP growth is below about 2.5% (generally considered fairly weak growth),<sup>60</sup> unsophisticated independents are significantly more likely than sophisticated independents to choose the “somewhat better” response, indicating that actual GDP growth influences unsophisticated independents’ retrospections less than those of sophisticated independents. Similarly, Figure 22 shows that unsophisticated independents are significantly more likely than sophisticated independents to erroneously report that the economy stayed “about the same” at negative GDP levels, which, by definition, indicate that the economy worsened over the past year.

It is also important to note that unsophisticated independents’ predicted probability lines closely resemble those of out-partisans: though some peaks occur outside of the feasible range of GDP values, like out-partisans, they appear to occur at higher GDP levels than they do for sophisticated independents in two of the three analyzed retrospection categories (“somewhat better” and “about the same”), indicating negatively biased retrospections. Thus, it is possible that unsophisticated independents take economic cues from out-partisans rather than evaluating objective data – though the data presented in the first micro-level model do not show any statistically significant group differences between sophisticated and unsophisticated independents’ retrospections.

### **Discussion: The Elusive Scorekeepers**

Unlike partisans who filter their perceptions through a “partisan screen” and unsophisticated independents who are information-deficient, sophisticated independents represent the unique characteristic of highly informed non-partisanship that perfectly matches the profile of Stimson’s scorekeepers. Stimson suggests this group is likely very small – on the

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<sup>60</sup> Most economists agree that 2-3% economic growth is considered marginal because it is just enough to keep up with inflation and population growth.

order of a few percent of the population. Indeed, sophisticated independents comprised only 2.4 percent of my sample. My empirical results confirm my theoretical expectations about how sophisticated independents should form economic retrospections compared to other groups: They use objective economic information the way Stimson's theory specifies "scorekeepers" should; their economic retrospections are more responsive to changes in GDP than any other group's retrospections. On both the micro and macro levels, sophisticated independents' retrospections fall between those of in- and out- partisans, suggesting that they report balanced economic evaluations.

Though I did not expect sophisticated and unsophisticated independents to exhibit systematic differences in mean retrospections on either the micro or macro levels, the lack of distinction between these groups in my results might lead scholars to question whether sophistication is a worthy distinction to make among independents. My GDP interaction results make the case for why it is: they clearly show that unsophisticated independents do not use GDP to inform their retrospections to the same degree that sophisticated independents do; when we analyze individual retrospection categories across levels of GDP, we see a clear difference in how sophisticated and unsophisticated independents formulate their retrospections. Since we are ultimately interested in how citizens form opinions about the economy, the sophistication distinction is indeed a worthy one. More work is necessary to ascertain the structure of unsophisticated independents' individual-level attitudes and the degree to which they engage in cue-taking that overcomes their informational deficiency. It is possible that unsophisticated independents' opinions are not truly random as Stimson (2004) proposes, particularly if this group takes cues from more informed groups.

If scorekeepers are readily identifiable through measurable characteristics (i.e., level of political information and lack of party identification), we will have taken a large step toward

bridging the paradox between micro- and macro-level political research findings which simultaneously assert that macro-level attitudes are systematic and orderly, but micro-level attitudes are not. The scorekeepers are important because they drive change in public opinion over time; they are a group of attentive and pragmatic, yet party-agnostic citizens who may be entirely responsible for overcoming what was once believed to be a damning individual-level empirical observation that Americans in large measure do not pay enough attention to politics to form intelligent political opinions. Future work should investigate whether there are other measurable characteristics of scorekeepers that can help us understand who they are and how they operate even more precisely. Though Stimson cautions that this group probably changes over time, there may still be characteristics that allow us to identify who scorekeepers are at any given moment. Future work should also investigate sophisticated independents on the macro-level to see if their opinions truly drive opinion dynamics. What ultimately remains to be determined is whether political scientists can normatively accept the idea that those who shape public opinion are a very small subset of the population who are not invested in the parties that structure politics.

## Tables

**Table 14. Group Differences in Retrospective Economic Evaluations**

<b>Predictor</b>	<b>Model 1</b>	<b>Model 2</b>
Sophisticated Independent	Baseline	Baseline
Unsophisticated Independent	-0.05(0.08)	0.73*(0.26)
In-Partisan	0.29*(0.09)	0.88*(0.21)
Out-Partisan	-0.24*(0.09)	0.52(0.19)
GDP	--	0.43*(0.10)
Unsophisticated Independent X GDP	--	-0.24*(0.09)
In-Partisan X GDP	--	-0.17*(0.07)
Out-Partisan X GDP	--	-0.23*(0.06)
Education	0.03(0.02)	0.03(0.02)
South	0.08*(0.04)	0.08(0.04)
Unemployed	-0.22*(0.03)	-0.26*(0.04)
Income	0.02(0.02)	0.02(0.02)
Black	-0.15(0.06)	-0.14*(0.06)
Male	0.18*(0.03)	0.17*(0.03)
1982	0.43*(0.01)	--
1984	1.65*(0.04)	--
1986	1.26*(0.03)	--
1990	0.37*(0.01)	--
1992	0.27*(0.01)	--
1994	1.42*(0.04)	--
1996	1.59*(0.04)	--
1998	1.83*(0.05)	--
2000	1.62*(0.04)	--
2004	1.05*(0.03)	--
2008	-0.48*(0.02)	--
Cut-Point 1 ( $\tau_1$ )	0.23(0.13)	0.68(.29)
Cut-Point 2 ( $\tau_2$ )	1.13(0.11)	1.49(0.26)
Cut-Point 3 ( $\tau_3$ )	2.18(0.12)	2.46(0.29)
Cut-Point 4 ( $\tau_4$ )	3.34(0.17)	3.57(0.33)
McFadden's Pseudo $R^2$	0.16	0.11
<b>Log-Pseudolikelihood</b>	<b>-21509.58</b>	<b>-22736.15</b>

**Notes:** Table entries are maximum likelihood ordered probit estimates with year cluster-robust standard errors in parentheses. In order to ensure a representative sample, data are weighted using ANES-provided post-stratification weights. N=17,090. \* $p < 0.05$  (two-tailed). Model 1 baseline year=2008. Data from 1988 are omitted because there is no post-election interview information assessment. Data from 2002 are omitted due to insufficient income data. Year dummies omitted in Model 2 due to perfect colinearity between GDP and year dummies.

**Table 15. Predicted Probabilities of Retrospection Categories by Group**

<b>Economic Retrospection Category</b>	<b>Sophisticated Independents</b>	<b>Unsophisticated Independents</b>	<b>In-Partisans</b>	<b>Out-Partisans</b>
1="much worse"	0.17	0.19	0.11	0.24
2="somewhat worse"	0.31	0.32	0.26	0.34
3= "about the same"	0.36	0.35	0.39	0.32
4 = "somewhat better"	0.14	0.13	0.20	0.10
5 = "much better"	0.01	0.01	0.03	0.01

**Notes:** Table entries are predicted probabilities of each economic retrospection category (displayed in the left-hand column) computed using the **prvalue** command from the **SPost** add-on package for Stata (Long & Freese 2005). When computing each group's predicted probabilities, all other group dummies are held at 0 and all other predictors are held at their means. Predicted probabilities may not sum to 1 within groups due to rounding error.

**Table 16. Group Differences in Predicted Probabilities of Retrospection Categories**

<b>Economic Retrospection Category</b>	<b>In-Partisans &amp; Out-Partisans</b>	<b>Unsophisticated Independents &amp; Sophisticated Independents</b>	<b>In-Partisans &amp; Sophisticated Independents</b>	<b>Out-Partisans &amp; Sophisticated Independents</b>
1="much worse"	-0.13*	+0.01	-0.06*	+0.07
2="somewhat worse"	-0.08	+0.01	-0.05	+0.03
3= "about the same"	+0.08*	-0.01	+0.03	-0.04*
4 = "somewhat better"	+0.11*	+0.01	+0.06*	-0.04*
5 = "much better"	+0.02*	+0.00	+0.02	-0.01

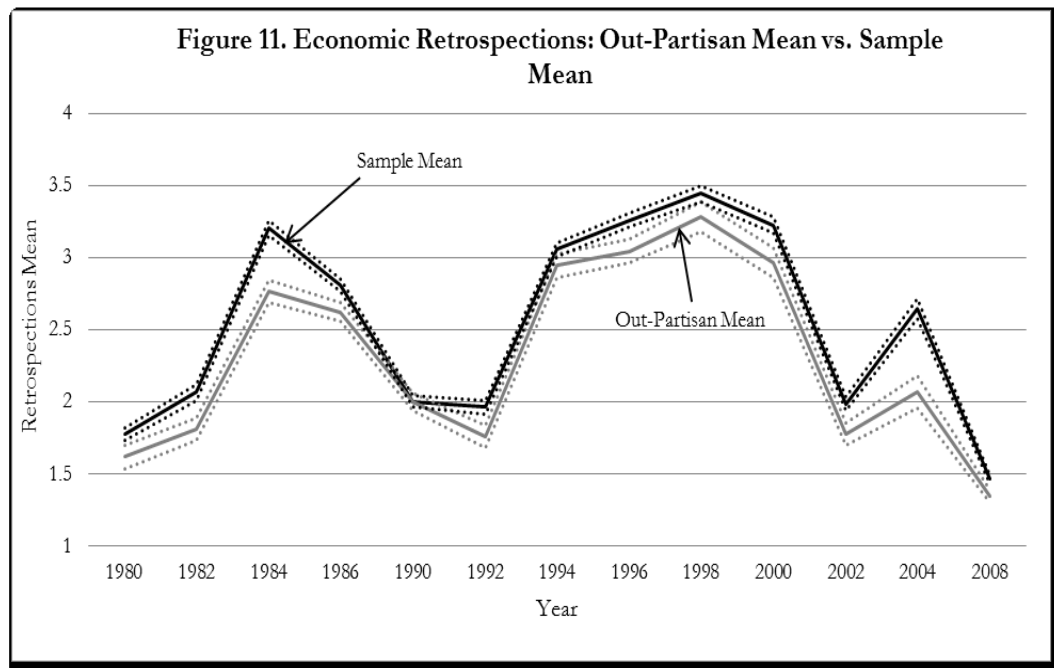
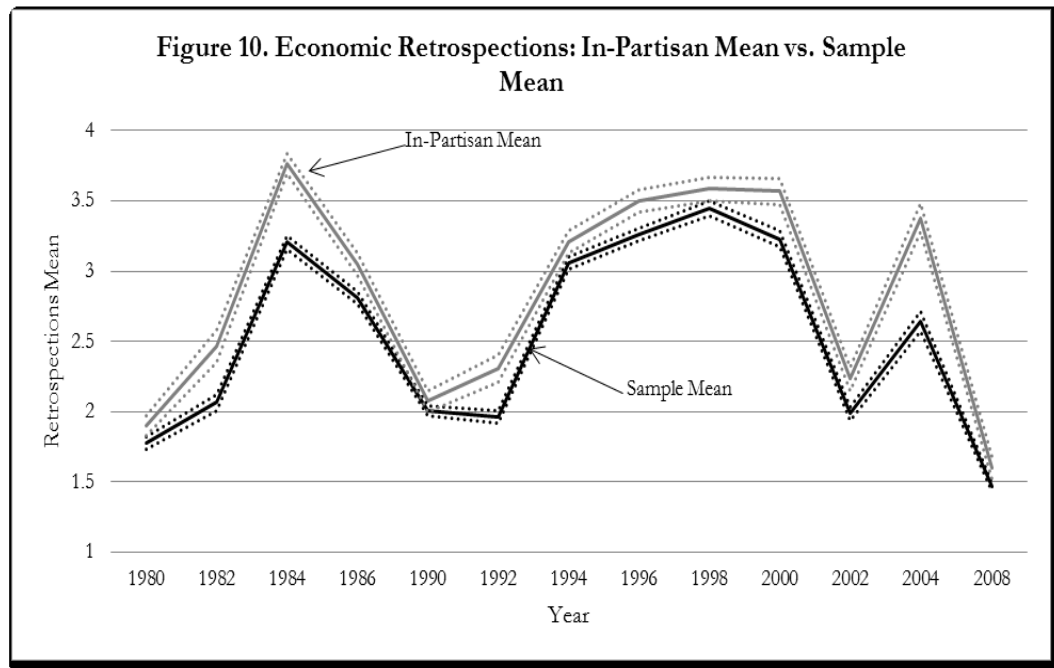
**Notes:** Table entries are between-group differences in the predicted probabilities reported in Table 15 for each economic retrospection category (displayed in the left-hand column). Predicted probabilities and their confidence intervals computed using the **prvalue** command from the **SPost** add-on package for Stata (Long & Freese 2005). The direction (sign) of the group differences is given moving from second-listed group to the first-listed group. When computing each group's predicted probabilities, all other group dummies are held at 0 and all other predictors are held at their means. \* $p < 0.05$  (two-tailed).

**Table 17. Group Deviations from Overall Sample Mean**

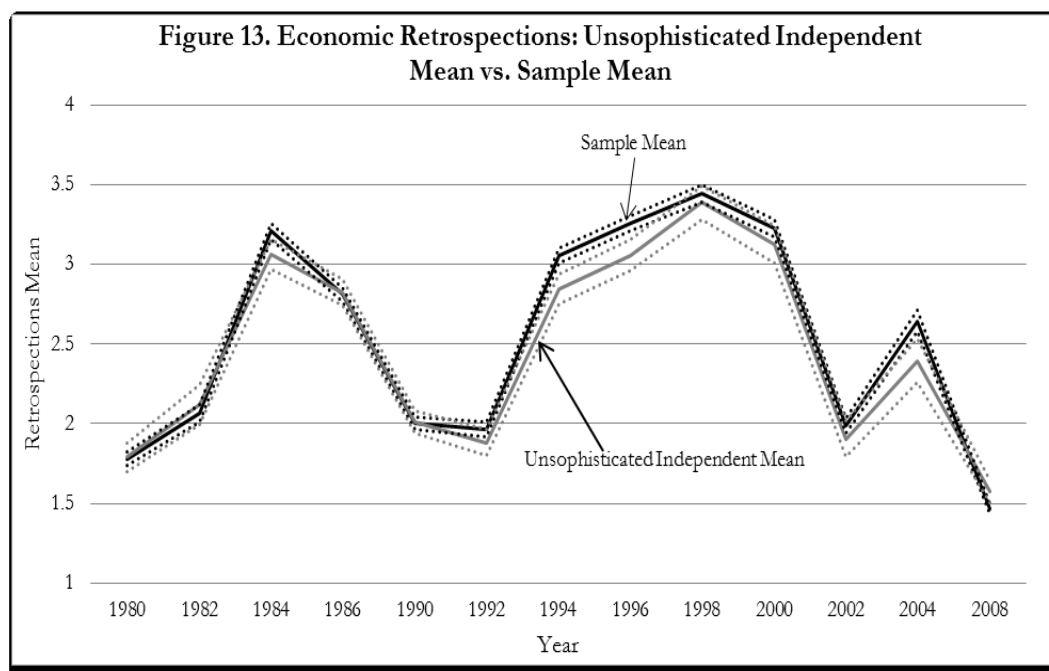
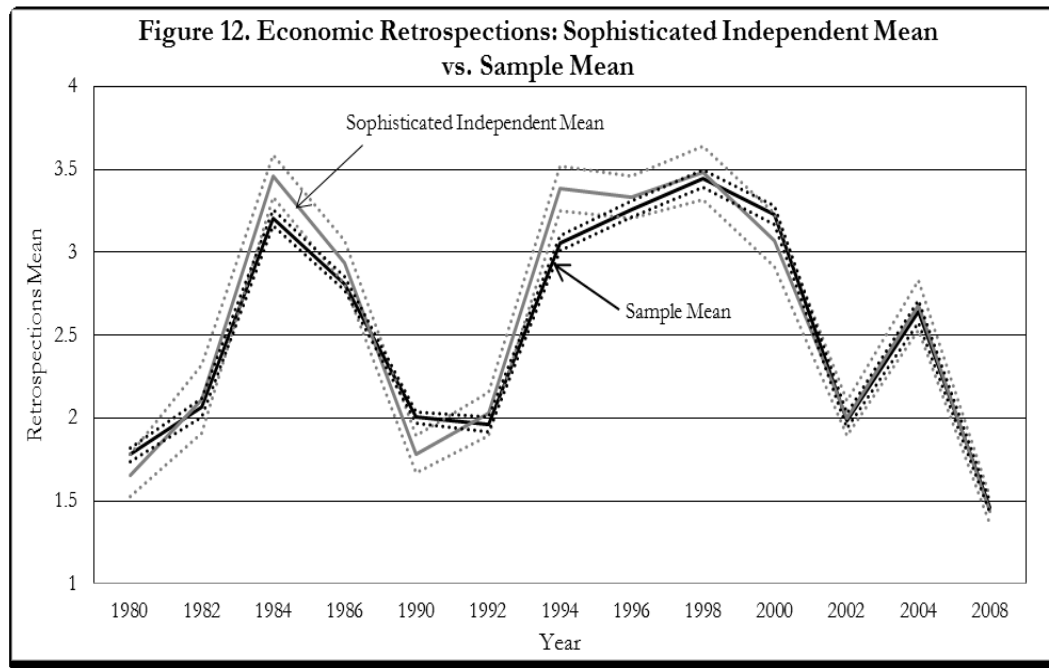
Year	Sophisticated Independents	Unsophisticated Independents	In-Partisans	Out- Partisans
1980	-0.12	0.01	0.12	-0.16
1982	0.05	0.05	0.40	-0.25
1984	0.25	-0.15	0.55	-0.44
1986	0.13	0.01	0.24	-0.19
1990	-0.22	0.01	0.07	0.00
1992	0.07	-0.08	0.35	-0.21
1994	0.33	-0.21	0.15	-0.11
1996	0.08	-0.20	0.24	-0.21
1998	0.04	-0.06	0.14	-0.16
2000	-0.16	-0.10	0.34	-0.26
2002	0.01	-0.08	0.25	-0.21
2004	0.04	-0.25	0.73	-0.58
2008	-0.02	0.11	0.14	-0.12
Total Deviation	1.50	1.33	3.72	2.90
Mean Deviation	0.12	0.10	0.29	0.22

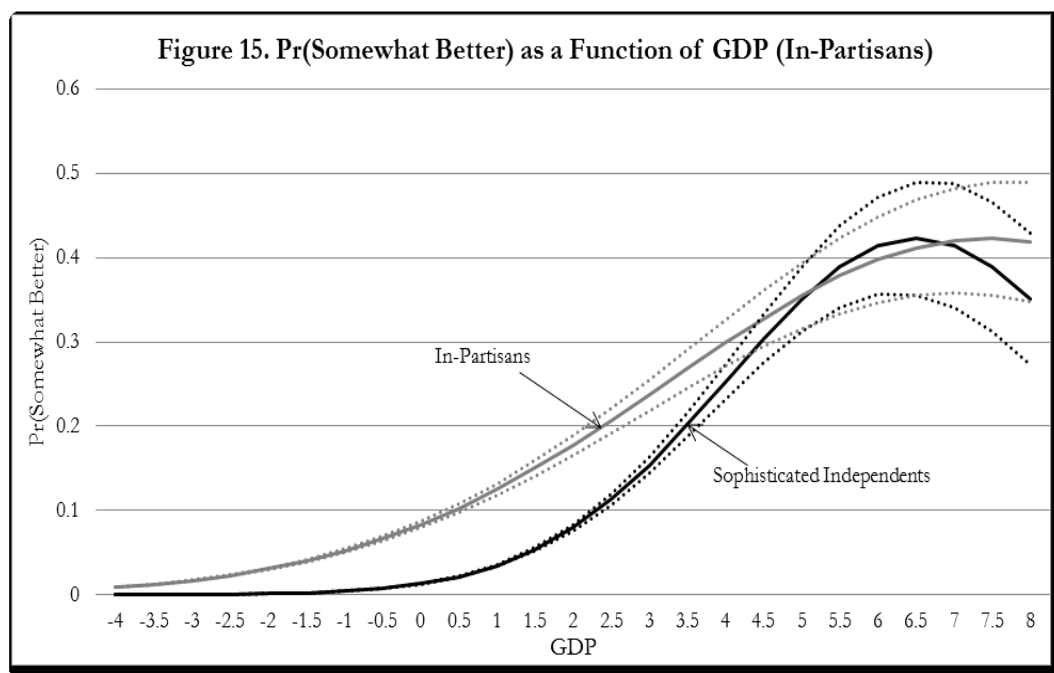
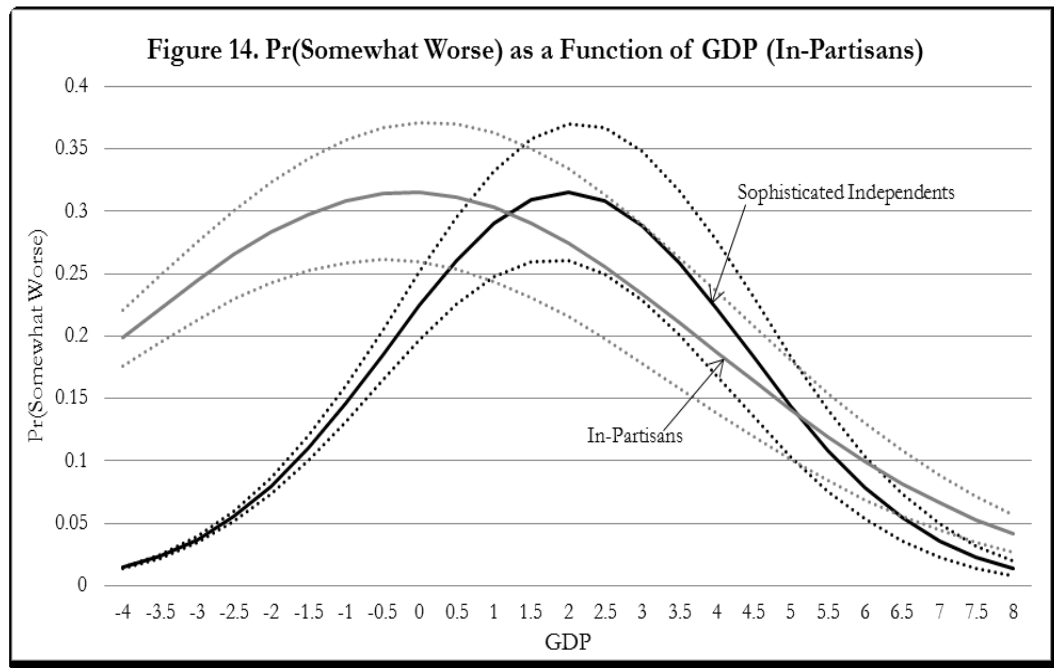
**Notes:** Total deviation is the sum of all absolute deviations from the sample mean across all survey years. Mean deviation is the mean absolute deviation from the sample mean.

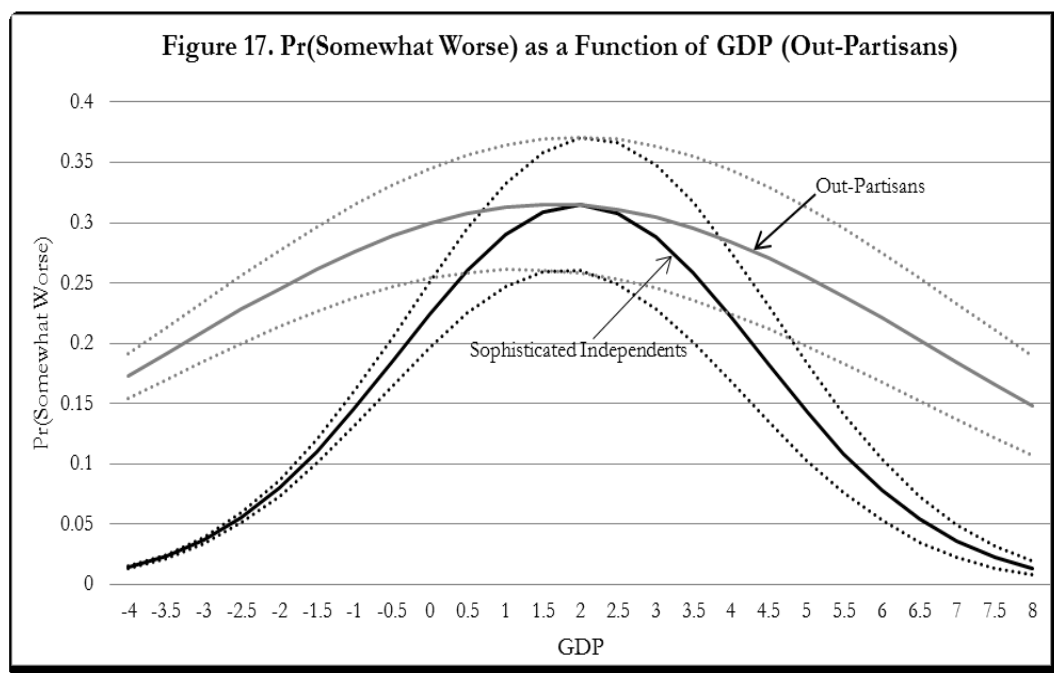
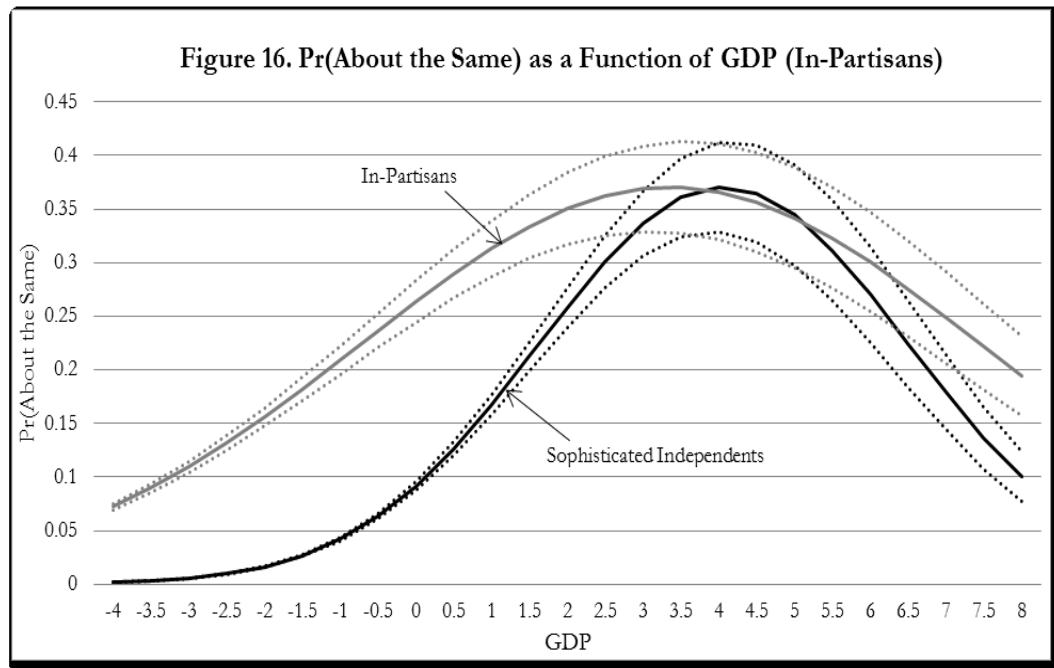
## Figures

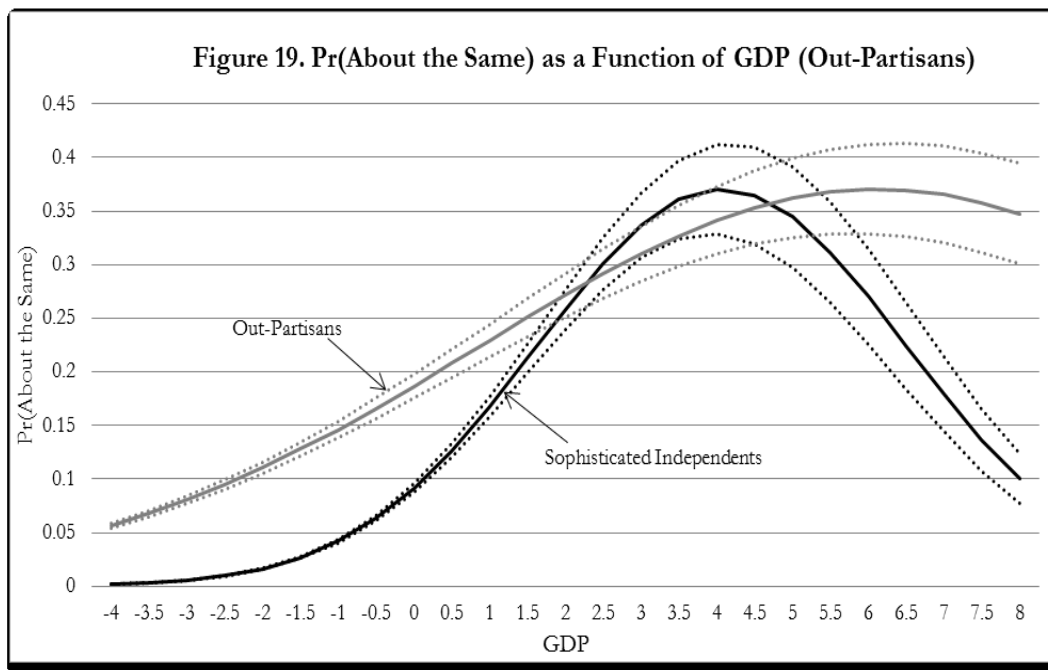
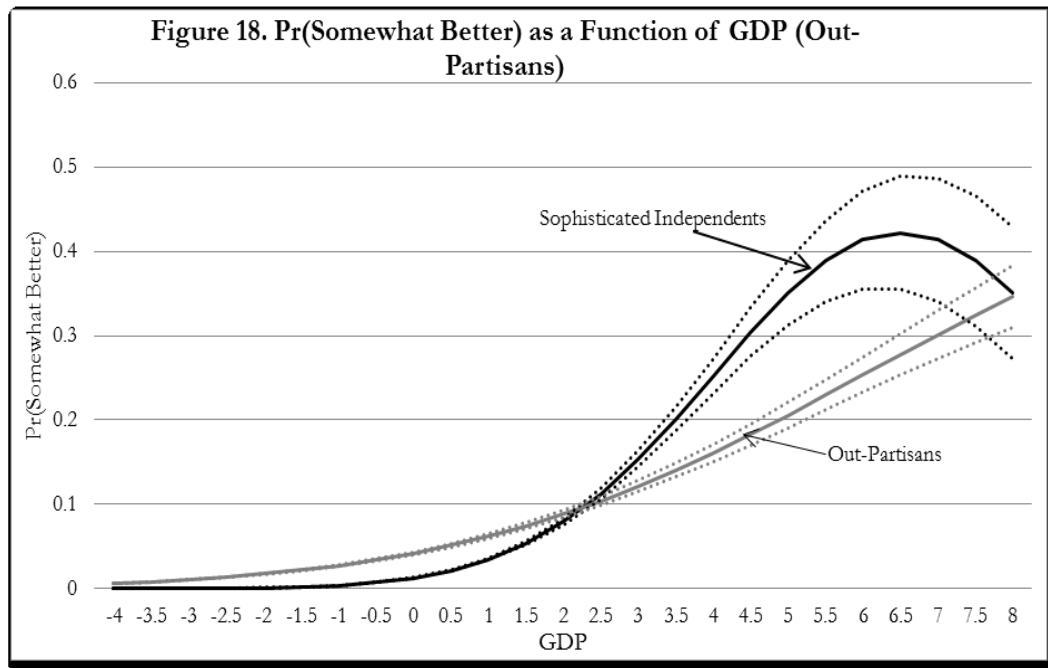


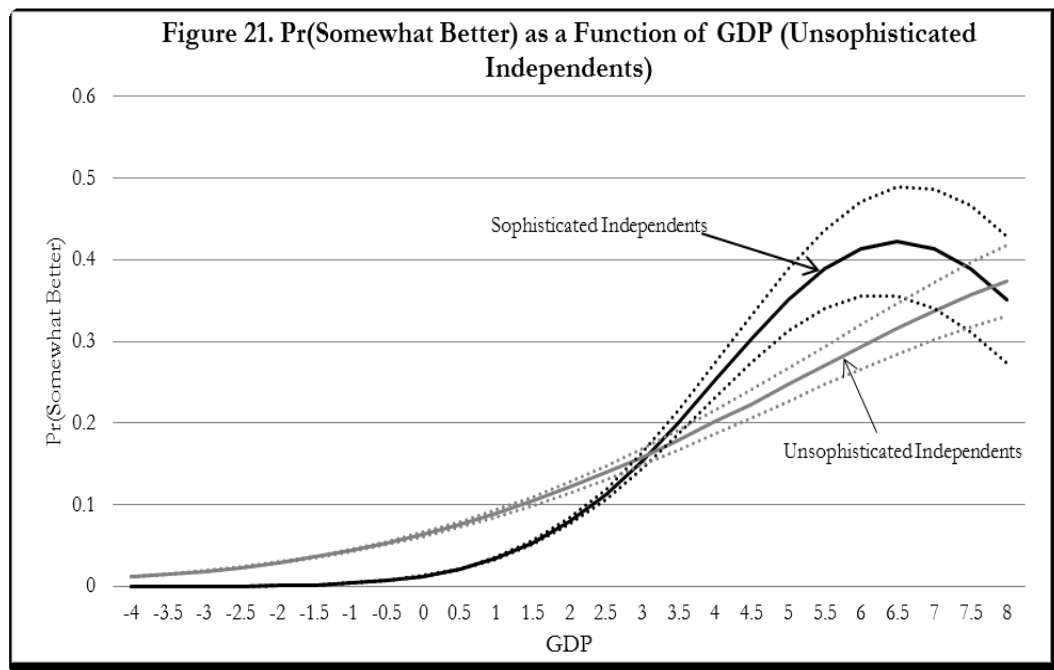
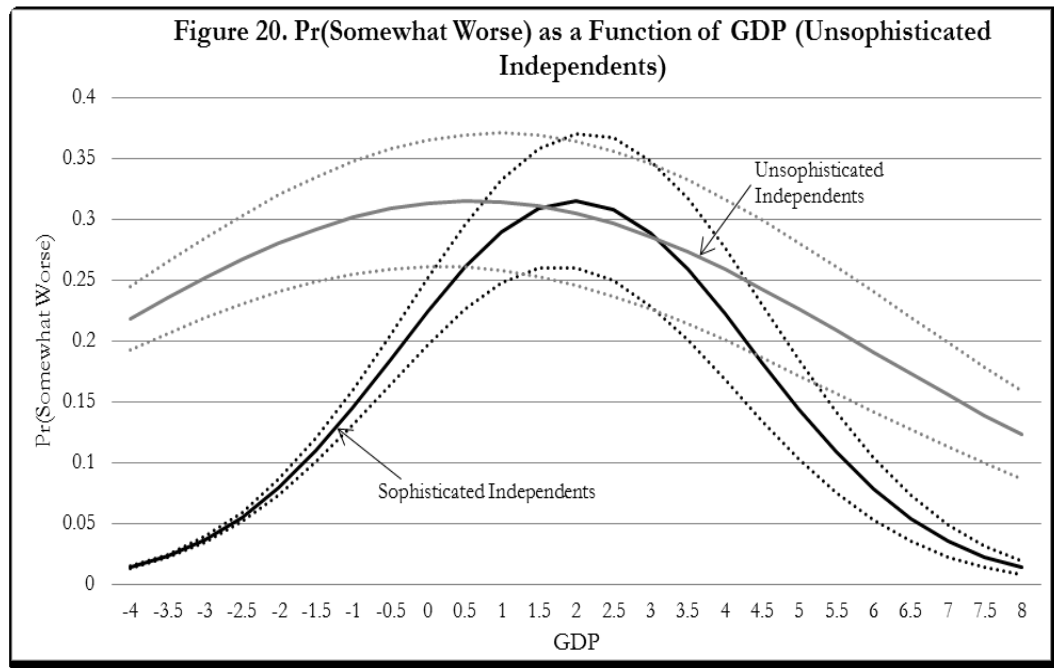


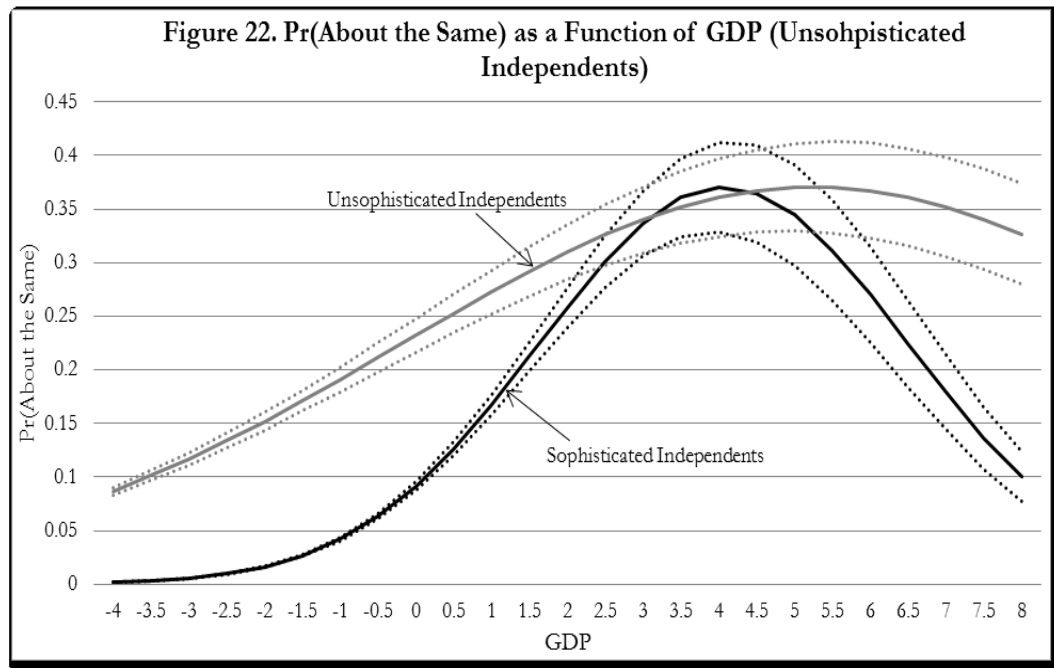












## APPENDICIES

### Appendix A. Supplementary Material for Chapter I.

**Table A. Dependent/Independent Variable Descriptive Statistics**

Variable	Obs	Mean	S.D.	Min	Max
Trust (100-point index)	8,904	32.29	21.85	0	100
Trust (dichotomous)	8,901	0.35	0.48	0	1
Perceived Polarization*	8,904	2.79	1.44	0	6
Ideological Alienation*	8,904	0.85	0.80	0	6

**Notes:** The 100-point trust index is composed of four ANES questions that measure (1) perceived corruption in government; (2) perceived wastefulness of government spending; (3) perception of big interests controlling government; (4) overall trust in government. The binary trust measure codes 0 as “none/only some of the time” and 1 as “most of the time/just about always.” \*These variables were not calculated for the 2002 ANES because their ancillary questions were not asked. Their descriptive statistics are identical for both model samples.

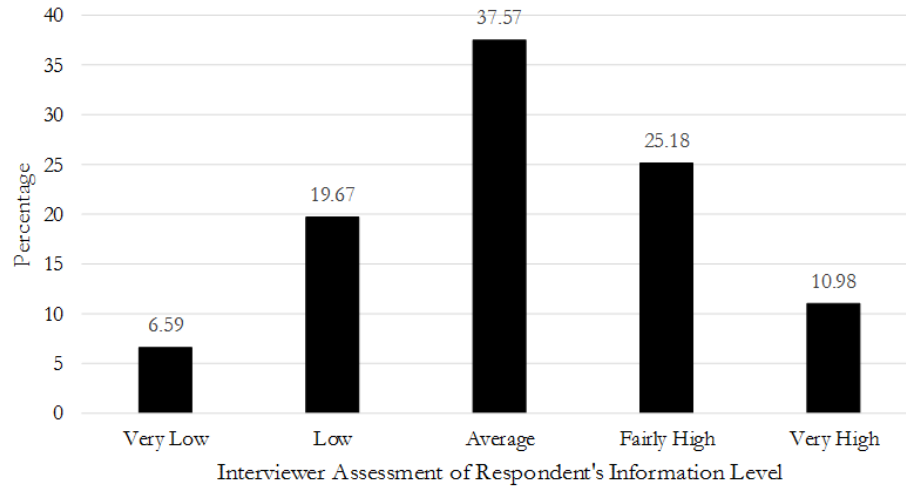
**Table B. Year Dummy Coefficient Estimates for Table 1<sup>^</sup>**

Year Dummy	OLS Estimate	ML Estimate
1980	Baseline	Baseline
1982	1.35 (1.16)	0.18 (0.14)
1984	4.20* (1.12)	0.40* (0.13)
1986	16.54* (1.16)	0.33* (0.14)
1988	3.03* (1.10)	0.39* (0.13)
1990	1.05 (1.05)	-0.04 (0.13)
1992	1.22 (0.97)	0.10 (0.12)
1994	-2.64* (1.10)	-0.51* (0.15)
1996	-0.46 (1.15)	-0.15 (0.14)
1998	1.87 (1.26)	0.11 (0.14)
2000	5.92* (1.48)	0.47* (0.17)
2004	6.56* (1.21)	0.66* (0.15)

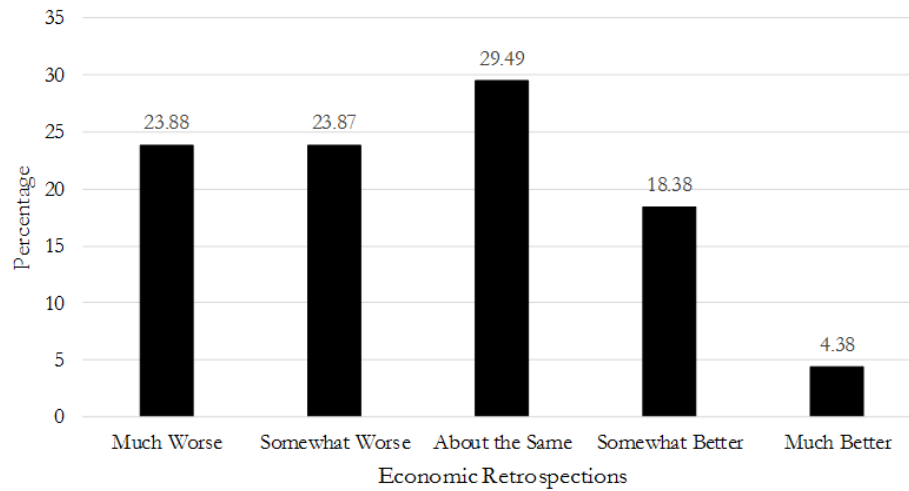
**Notes:** The dependent variable in the OLS estimation is trust in government measured using the ANES trust index scaled from 0 for “least trusting” to 100 for “most trusting.” The dependent variable in the ML estimation is trust in government measured using a binary scale of 1 for “trust” and 0 for “do not trust.” Table entries are OLS/ML estimates with robust standard errors in parentheses. Full model in Table 1. ANES supplied weights used to ensure data accurately represent population. \* $p < 0.05$  (two-tailed);  $N = 8,904$  (OLS);  $N = 8,901$  (ML). <sup>^</sup>2002 not included.

Appendix B. Supplementary Material for Chapter III.

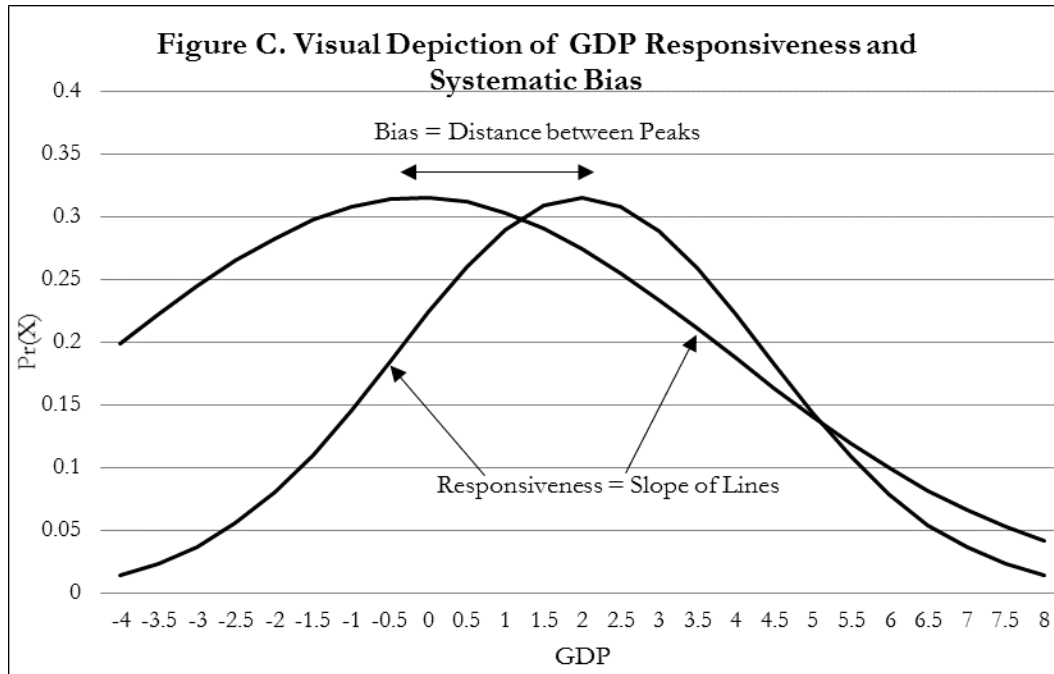
**Figure A. Sampling Distribution of Interviewer Assessment of Respondent's Information Level**



**Figure B. Sampling Distribution of Economic Retrospections**







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